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ART. IX.—*Principal Characters of American Jurassic Dinosaurs.* Part VI: *Restoration of Brontosaurus*, (with plate I); by Professor O. C. MARSH.

IN the previous articles of this series, the writer has given the more important characters of the order *Sauropoda.** A volume on this group is now in preparation, and the illustrations (90 plates) are nearly completed. One of these is a restoration of *Brontosaurus*, which has so many points of interest that a reduced figure is here presented. Several new characters of this group are added, some of which will be of interest to comparative anatomists.

RESTORATION OF BRONTOSAURUS, (Plate I.)

Nearly all the bones here represented belonged to a single individual, which when alive was nearly or quite fifty feet in length. The position here given was mainly determined by a careful adjustment of these remains. That the animal at times assumed a more erect position than here represented is probable, but locomotion on the posterior limbs alone was hardly possible.

The head was remarkably small. The neck was long, and, considering its proportions, flexible, and was the lightest portion of the vertebral column. The body was quite short, and the abdominal cavity of moderate size. The legs and feet were massive, and the bones all solid. The feet were plantigrade,

* This Journal, xvi, 411, Nov. 1878; xvii, 86, Jan. 1879; xxi, 417, May, 1881; and xxiii, 81, Jan. 1882.

and each foot-print must have been about a square yard in extent. The tail was large, and nearly all the bones solid.

The diminutive head will first attract attention, as it is smaller in proportion to the body than in any vertebrate hitherto known. The entire skull is less in diameter or actual weight than the fourth or fifth cervical vertebra.

A careful estimate of the size of *Brontosaurus*, as here restored, shows that when living the animal must have weighed more than twenty tons. The very small head and brain, and slender neural cord, indicate a stupid, slow moving reptile. The beast was wholly without offensive or defensive weapons, or dermal armature.

In habits, Brontosaurus was more or less amphibious, and its food was probably aquatic plants or other succulent vegetation. The remains are usually found in localities where the animals had evidently become mired.

Among the new points in the skull of the *Sauropoda* recently determined are the following:

Pituitary Fossa.

In *Morosaurus*, the pituitary fossa is comparatively shallow, much like that in the crocodile, and many birds, being connected with the under surface of the skull by the two usual divergent foramina for the passage of the internal carotid arteries. In *Apatosaurus*, however, it is remarkably different. Here the fossa becomes enlarged into a vertical canal, which, expanding below, communicates by a wide transverse orifice with the pharyngeal cavity. The arterial foramina are here canals thinly covered over with bone, and open just within the rim of the lower orifice. The pituitary cavity itself has a firm smooth wall throughout. The openings are both transverse, and oval in shape. The upper one is eighteen by six millimeters in its diameters; the lower opening thirty by twelve.

This remarkable connection of the cerebral cavity with the alimentary canal is an embryonic character, and corresponds to the condition observed in the chick at the fifth day of incubation. This peculiar feature appears to be a family character of the *Atlantosauridæ*.

Post-occipital Bones.

In two genera of the *Sauropoda*, (*Morosaurus* and *Brontosaurus*), and probably in all members of this order, there is a pair of small bones connected with the skull which have not hitherto been observed in any vertebrates. These bones, which may be called the *post-occipital* bones, were found in position in one specimen, and with the skull in several others. When in

place, they are attached to the occiput just above the foramen magnum, and extend backward and outward, overlapping the lateral pieces of the atlas, thus protecting the spinal cord at this point, which would otherwise be much exposed.

These bones are short, flattened, and slightly curved, resembling somewhat a riblet. The anterior end is thickened and rugose for attachment to a roughened surface on the exoccipital, just above and outside the foramen magnum. The shaft is flattened from above downward, and gradually converges to a thin posterior end. In *Morosaurus grandis*, these bones are about 65 mm. in length, and 30 along the surface which joins the occiput. They correspond in position to the muscle in mammals known as the *rectus capitis posticus minor*.

In the existing Cormorants (*Graculus*) a single slender bone is articulated to the occiput on the median line. This, however, does not correspond to the bones here described. To distinguish it from the post-occipitals, it may be called the *nuchal* bone.

Stapes wanting.

In the skull of *Morosaurus* in which the post-occipital bones were found in position and the other bones at the base of the skull were undisturbed, a careful search was made for the stapes, but no indication of it was found. Its absence in this specimen, so well preserved, would indicate that it was wanting in this genus, if not in the other Sauropoda.

Columella present.

In a skull of *Brontosaurus* in which the bones, although displaced, were in very perfect preservation, a pair of bones were found which apparently are the columellæ. They are elongated, flattened bones, with the shaft somewhat constricted in the middle, and twisted. Their length corresponds to the elevated posterior part of the skull in this genus.

Hyoid bones.

There are two pairs of hyoid bones in the Sauropoda. They are elongated, rodlike, and somewhat curved. In *Brontosaurus excelsus*, they are 210 and 130 mm. in length respectively.

Among the other points of interest in the skull of the *Sauropoda* are the following:

The parietal bones are very short, and form but a small portion of the brain case. They are composed chiefly of the flattened arched processes, which meet with the squamosals at their outer ends. There is no parietal foramen. The squamosals lie upon the par-occipital processes. They have a

short deep groove for the reception of the post-frontal. On their lower part, which descends in front of the par-occipital process, they expand into a thin spoon-shaped form, which fits over the head of the quadrate. The quadrate has an oval rounded head, and slender shaft. Below, it is firmly united to the pterygoids. On the outer side, the quadrato-jugals are attached. These bones are elongate, and slightly sigmoid in shape. The lower end is rodlike, and curved forward, descending below the articular surface of the quadrate. The pterygoids are tri-radiate bones, with the posterior ends cup-shaped, resembling the partially closed human hand. This cavity, somewhat restricted by a thumb-like process, receives the basi-pterygoid process.

The Vertebræ.

There are twenty-seven precaudal vertebræ in *Brontosaurus*, of which the first twelve bear pleurapophyses, or hatchet bones, united to the centra, and may hence be called true cervicals. Of the remaining twelve which bear free ribs, the thirteenth, fourteenth, and fifteenth have the surface for the articulation of the head of the rib on the centrum, below the neural suture.

All the precaudal vertebræ have large cavities in the centrum, communicating exteriorly with the surface by means of large lateral foramina. This cavernous structure of the vertebræ gradually decreases posteriorly, until in the anterior caudal vertebræ it is confined to a small pocket above the transverse process. The neural arches of the presacral vertebræ contain numerous deep cavities. The pleurapophyses of the cervical vertebræ are also reticulate in their structure, and some of the anterior ribs have small but deep fossæ below the tubercle.

Post-metapophyses.

On the last two or three cervical vertebræ of *Brontosaurus*, there is a convoluted ridge of bone over the posterior zygapophyses. In the anterior dorsals, this ridge becomes stronger and more elongated, forming a distinct protuberance. These processes have not hitherto been described. As they are analogous to the processes in mammals known as metapophyses, they may bear the same general name, being distinguished as the post-metapophyses. The term pre-metapophyses should then be applied to the processes in mammals.

The post-metapophyses probably serve for the attachment of ligaments in the place of the neural spine, which is here wholly wanting. These processes, which are at first oblique in position, gradually become more vertical and stouter, and, coalescing at their bases, finally become united throughout, and are thus converted into the neural spine.

Fœtal Dinosaurs.

Remains of a very small dinosaur were found in immediate relation with the type specimen of *Morosaurus grandis*. These remains, which consist of a complete femur, the larger portion of both humeri, and several vertebræ, show no essential differences from the large specimens except in size, and indicate an animal of perhaps seven feet in length, and little more than two feet in height. The imperfect ossification of these bones indicates that the animal was very young, and it seems probable that it was fœtal. The only other similar case known in the *Dinosauria* is the apparent embryo observed by the writer in *Compsognathus*.*

Classification.

The various genera of the *Sauropoda*, and in fact of the *Dinosauria* in general, cannot at present be distinguished by the detached teeth. In one form, however, the teeth are quite peculiar, and the dentition appears to offer generic characters. The maxillary teeth of this form have been referred to *Stegosaurus* (this Journal, vol. xix, p. 255; pl. VI, figs 4 and 5), as they were first found in connection with the remains of that genus. Later investigations indicate that they belong to the *Sauropoda*, and there is some evidence that they are the teeth of *Diplodocus*.

The main characters of the order *Sauropoda*, and of the two families now known to belong to it, are as follows:

Order Sauropoda. Herbivorous.

Feet plantigrade, ungulate; five digits in manus and pes; second row of carpal and tarsal bones unossified. Pubes projecting in front, and united distally by cartilage; no post-pubis. Fore and hind limbs nearly equal; limb bones solid. Sternal bones parial. Premaxillary bones with teeth. Precaudal vertebræ hollow. Each sacral vertebra supports its own transverse processes.

Family *Atlantosauridæ*. Anterior vertebræ opisthocœlian. Ischia directed downward, with extremities meeting on median line. Anterior caudals with lateral cavities. A pituitary canal.

Family *Morosauridæ*. Anterior vertebræ opisthocœlian. Ischia directed backward, with sides meeting on median line. Anterior caudals solid. Pituitary fossa only.

Yale College, New Haven, July 12th, 1883.

* This Journal, xxii, 340, November, 1881.

ART. X.—*The Evolution of the American Trotting-Horse;* by FRANCIS E. NIPHER.

SINCE my paper in the July number of this Journal was written, I have calculated the constants in the differential equation by mathematical methods, and have obtained a result differing slightly from that given in the July number. The most probable value for the minimum time of trotting a mile turns out to be 91 seconds, instead of 93 seconds as was obtained by graphical methods.

The final equation is

$$\log (s-91) = 1{\cdot}703-0{\cdot}0046\,T,$$

where s is the time (in seconds) of trotting a mile, and T is estimated in years from 1860.

This equation does not give essentially different results from the former one, the only point of interest being in the new value for the limiting time. The probable error of this value is not over four seconds, and it is not likely that the running horse will beat his present record by five seconds, so that it is very probable that the trotter will finally surpass the running horse.

This conclusion does not rest solely on mathematical evidence. The trotter carries his body more steadily—with less of rise and fall—than the runner, and it seems very reasonable that this should result to the advantage of the trotter, when the process of developing and adjusting his muscles and chest shall have been sufficiently carried on, so that the contest between the two animals shall have been reduced to a matter of muscular capacity.

It is well known that some herds of wild horses on the Texas plains were natural pacers, and even when pushed to the utmost and for days together by the best running horses, they nearly all held their gait. One large white pacer became widely known and many attempts were made to capture him, but he proved more than a match for the best horses that could be brought against him.

The record of the trotting-horse during the next ten years will enable us to determine the limiting speed with sufficient precision to settle the matter beyond question. It is even now evident that our good ancestors, who thought it wicked to run horses, builded more wisely than they knew when they began the evolution of the American trotting-horse.

ART. XI.—*On Concave Gratings for Optical Purposes;** by HENRY A. ROWLAND, Professor of Physics, Johns Hopkins University, Baltimore.

GENERAL THEORY.

HAVING recently completed a very successful machine for ruling gratings, my attention was naturally called to the effect of irregularity in the form and position of the lines and the form of the surface on the definition of the grating. Mr. C. S. Peirce has recently shown, in the American Journal of Mathematics, that a periodic error in the ruling produces what have been called ghosts in the spectrum. At first I attempted to calculate the effect of other irregularities by the ordinary method of integration, but the results obtained were not commensurate with the labor. I then sought for a simpler method. Guided by the fact that inverse methods in electrical distribution are simpler than direct methods, I soon found an inverse method for use in this problem.

In the use of the grating in most ordinary spectroscopes, the telescopes are fixed together as nearly parallel as possible, and the grating turned around a vertical axis to bring the different spectra into the field of view. The rays striking on the grating are nearly parallel, but for the sake of generality I shall assume that they radiate from a point in space and shall investigate the proper ruling of the grating to bring the rays back to the point from which they started. The wave fronts will be a series of spherical shells at equal distances apart. If these waves strike on a reflecting surface, they will be reflected back provided they can do so all in the same phase. A sphere around the radiant point satisfies the condition for waves of all lengths and thus gives the case of ordinary reflection. Let any surface cut the wave surfaces in any manner and let us remove those portions of the surface which are cut by the wave surfaces: the light of that particular wave-length can then be reflected back along the same path in the same phase and thus, by the above principle, a portion will be sent back. But

* An abstract of this paper with some other matter was given at the Physical Society of London in November last, the paper being in my hand in its present shape at that time. As I wished to make some additions, for which I have not yet had time, I did not then publish it. I was much surprised soon after to see an article on this subject, which had been presented to the Physical Society and was published in the Philosophical Magazine. The article contains nothing more than an extension of my remarks at the Physical Society and formulæ similar to those in this paper. As I have not before this published anything except a preliminary notice of the concave gratings, I expected a little time to work up the subject, seeing that the practical work of photographing the spectrum has recently absorbed all my time. But probably I have waited too long.

the solution holds for only one wave-length and so white light will be drawn out into a spectrum. Hence we have the important conclusion that a theoretically perfect grating for one position of the slit and eyepiece can be ruled on *any* surface, flat or otherwise. This is an extremely important practical conclusion and explains many facts which have been observed in the use of gratings. For we see that errors of the dividing engine can be counterbalanced by errors in the flatness of the plate, so that a bad dividing engine may now and then make a grating which is good in one spectrum but not in all. And so we often find that one spectrum is better than another. Furthermore Professor Young has observed that he could often improve the definition of a grating by slightly bending the plate on which it was ruled.

From the above theorem we see that if a plate is ruled in circles whose radius is $r \sin \mu$ and whose distance apart is $\Delta r \sin \mu$, where Δr is constant, then the ruling will be appropriate to bring the spectrum to a focus at a distance, r, and angle of incidence, μ. Thus we should need no telescopes to view the spectrum in that particular position of the grating. Had the wave surfaces been cylindrical instead of spherical the lines would have been straight instead of circular, but at the above distances apart. In this case the spectrum would have been brought to a focus, but would have been diffused in the direction of the lines. In the same way we can conclude that in flat gratings any departure from a straight line has the effect of causing the dust in the slit and the spectrum to have different foci, a fact sometimes observed.

We also see that, if the departure from equal spaces is small, or, in other words, the distance r is great, the lines must be ruled at distances apart represented by

$$c\left(1-\frac{\cos^2 \mu}{r \sin \mu}x+\&c.\right)$$

in order to bring the light to a focus at the angle μ and distance r, c being a constant and x the distance from some point on the plate. If μ changes sign, then r must change in sign. Hence we see that the effect of a linear error in the spacing is to make the focus on one side shorter and the other side longer than the normal amount. Professor Peirce has measured some of Mr. Rutherfurd's gratings and found that the spaces increased in passing along the grating, and he also found that the foci of symmetrical spectra were different. But this is the first attempt to connect the two. The definition of a grating may thus be very good even when the error of run of the screw is considerable, provided it is linear.

Concave Gratings.

Let us now take the special case of lines ruled on a spherical surface. And let us not confine ourselves to light coming back to the same point, but let the light return to another point, let the coördinates of the radiant point and focal point be $y=0$, $x=-a$ and $y=0$, $x=+a$, and let the center of the sphere whose radius is ρ be at x', y'. Let r be the distance from the radiant point to the point x, y, and let R be that from the focal point to x, y. Let us then write

$$2b=\mathrm{R}+rc.$$

Where c is equal to ± 1 according as the reflected or transmitted ray is used. Should we increase b by equal quantities and draw the ellipsoids or hyperboloids so indicated, we could use these surfaces in the same way as the wave surfaces above. The intersections of these surfaces with any other surface form what are known as Huyghen's zones. By actually drawing these zones on the surface, we form a grating which will diffract the light of a certain wave-length to the given focal point. For the particular problem in hand, we need only work in the plane x, y for the present.

Let s be an element of the curve of intersection of the given surface with the plane x, y. Then our present problem is to find the width of Huyghen's zones on the surface, that is ds in terms of db.

The equation of the circle is

$$(x-x')^2+(y-y')^2=\rho^2$$

and of the ellipse or hyperbola

$$\mathrm{R}+rc=2b$$

or
$$(b^2-a^2)\,x^2+b^2y^2=b^2\,(b^2-a^2)$$

in which c has disappeared

$$ds=\sqrt{dx^2+dy^2}\,;\quad \frac{dx}{dy}=-\frac{y-y'}{x-x'}$$

$$dx\left\{(b^2-a^2)\,x-b^2y\,\frac{x-x'}{y-y'}\right\}=b\{2b^2-(x^2+y^2+a^2)\}\,db$$

$$dy\left\{-(b^2-a^2)\,x\,\frac{y-y'}{x-x'}+b^2y\right\}=b\{2b^2-(x^2+y^2+a^2)\}\,db$$

$$\therefore\ \frac{ds}{db}=\rho b\,\frac{2b^2-(x^2+y^2+a^2)}{(b^2-a^2)\,(y-y')\,x-b^2\,(x-x')y}$$

This equation gives us the proper distance of the rulings on the surface, and if we could get a dividing engine to rule according to this formula the problem of bringing the spectrum to a

focus without telescopes would be solved. But an ordinary dividing engine rules equal spaces and so we shall further investigate the question whether there is any part of the circle where the spaces are equal. We can then write

$$\frac{ds}{db}=\mathrm{C}.$$

And the differential of this with regard to an arc of the circle must be zero. Differentiating and reducing by the equations

$$\frac{dx}{dy}=-\frac{y-y'}{x-x'};\quad \frac{db}{dy}=-\frac{\rho}{\mathrm{C}(x-x')}$$

we have

$$\rho\left\{2xb(y-y')-2yb(x-x')-\frac{\rho}{\mathrm{C}}[6b^2-(x^2+y^2+a^2)]\right\}$$

$$+\mathrm{C}\left\{(y-y')[(b^2-a^2)(y-y')-b^2y]-(x-x')[(b^2-a^2)x-b^2(x-x')]\right.$$

$$\left.+\frac{2b\rho}{\mathrm{C}}[x(y-y')-y(x-x')]\right\}=0.$$

It is more simple to express this result in terms of R, r, ρ and the angles between them.

Let μ be the angle between ρ and r, and ν that between ρ and R. Let us also put

$$\alpha=\frac{\mu-\nu}{2}\quad\text{and}\quad\varepsilon=\frac{\mu+\nu}{2}.$$

Let β, γ and δ also represent the angles made by r, R and ρ respectively with the line joining the source of light and focus, and let

$$\eta=\frac{\beta+\gamma}{2}.$$

Then we have

$$x=\frac{\mathrm{R}\cos\gamma+r\cos\beta}{2};\quad y=\frac{\mathrm{R}\sin\gamma+r\sin\beta}{2};\quad a=\frac{r\cos\beta-\mathrm{R}\cos\gamma}{2}$$

$$(b^2-a^2)(y-y')^2+b^2(x-x')^2=\rho^2(b^2-a^2\sin^2\delta)$$

$$b^2-a^2=\mathrm{R}r\cos^2\alpha$$

$$\sin\eta=\frac{\mathrm{R}+r}{2a}\sin\alpha;\quad\cos\eta=\frac{r-\mathrm{R}}{2a}\cos\alpha$$

$$\mathrm{R}=b-\frac{a}{b}x;\quad r=b+\frac{a}{b}x$$

$$x=b\frac{\cos\eta}{\cos\alpha};\quad y=a\frac{\sin\gamma\sin\beta}{\sin\alpha\cos\alpha}=\frac{\mathrm{R}r}{b}\sin\eta\cos\alpha$$

$$b^2y(y-y')+x(b^2-a^2)(x-x')=\frac{b\mathrm{R}r\rho}{2}(\cos\mu+\cos\nu)$$

$$2b^2-(x^2+y^2+a^2)=\mathrm{R}r$$

$$x(b^2-a^2)(y-y')-b^2y(x-x')=\frac{\mathrm{R}rb\rho}{2}(\sin\mu+\sin\nu)$$

$$\mathrm{C}=\frac{2}{\sin\mu+\sin\nu}=\frac{1}{\cos\alpha\sin\varepsilon}$$

$$2a\cos\delta=r\cos\mu-\mathrm{R}\cos\nu$$

$$2a\sin\delta=r\sin\mu-\mathrm{R}\sin\nu.$$

On substituting these values and reducing, we find

$$\rho^*=\frac{2\mathrm{R}r\cos\alpha\cos\varepsilon}{r\cos^2\nu+\mathrm{R}\cos^2\mu}.$$

Whence the focal length is

$$r=\frac{\rho\mathrm{R}\cos^2\mu}{2\mathrm{R}\cos\alpha\cos\varepsilon-\rho\cos^2\nu}.$$

For the transmitted beam, change the sign of R. Supposing ρ, R and ν to remain constant and r and μ to vary, this equation will then give the line on which all the spectra and the central image are brought to a focus.

By far the most interesting case is obtained by making

$$r=\rho\cos\mu \quad \mathrm{R}=\rho\cos\nu,$$

since these values satisfy the equation. The line of foci is then a circle with a radius equal to one-half ρ. Hence if a

* A more simple solution is the following; $\frac{ds}{db}$ must be constant in the direction in which the dividing engine rules. If the dividing engine rules in the direction of the axis, y, the differential of this with respect to y must be zero. But we can also take the reciprocal of this quantity and so we can write for the equation of condition

$$\frac{d}{dy}\frac{d(\mathrm{R}+r)}{ds}=0.$$

Taking a circle as our curve we can write

$$(x-x')^2+(y-y')^2=\rho^2$$

and

$$(x-x'')^2+(y-y'')^2=\mathrm{R}^2$$

$$(x-x''')^2+(y-y''')^2=r^2$$

$$\frac{d(\mathrm{R}+r)}{ds}=\frac{1}{\rho}\left\{(y-y')\left(\frac{x-x''}{\mathrm{R}}+\frac{x-x'''}{r}\right)+(x-x')\left(\frac{y-y''}{\mathrm{R}}+\frac{y-y'''}{r}\right)\right\}$$

$$\frac{d}{dy}\frac{d(\mathrm{R}+r)}{ds}=\frac{1}{\rho}\left\{\frac{x-x''}{\mathrm{R}}+\frac{x-x'''}{r}-(y-y')\left[\frac{(x-x'')(y-y'')}{\mathrm{R}^3}+\frac{(x-x''')(y-y''')}{r^3}\right]\right.$$

$$\left.+(x-x')\left[\frac{x-x'')^2}{\mathrm{R}^3}+\frac{(x-x''')^2}{r^3}\right]\right\}=0.$$

Making

$$x=0,\ y=0,\ y'=0,\ x'=\rho$$

we have

$$\frac{x''}{\mathrm{R}}+\frac{x'''}{r}-\rho\left(\frac{x''^2}{\mathrm{R}^3}+\frac{x'''^2}{r^3}\right)=0,$$

or

$$\rho=\mathrm{R}r\frac{\cos\mu+\cos\nu}{r\cos^2\nu+\mathrm{R}\cos^2\mu}=\frac{2\mathrm{R}r\cos\alpha\cos\varepsilon}{r\cos^2\nu+\mathrm{R}\cos^2\mu}.$$

source of light exist on this circle, the reflected image and all the spectra will be brought to a focus on the same circle. Thus if we attach the slit, the eye-piece and the grating to the three radii of the circle, however we move them, we shall always have some spectrum in the focus of the eye-piece. But in some positions the line of foci is so oblique to the direction of the light that only one line of the spectrum can be seen well at any one time. The best position of the eye-piece as far as we consider this fact is thus the one opposite to the grating and at its center of curvature. In this position the line of foci is perpendicular to the direction of the light, and we shall show presently that the spectrum is normal at this point whatever the position of the slit, provided it is on the circle.

1.

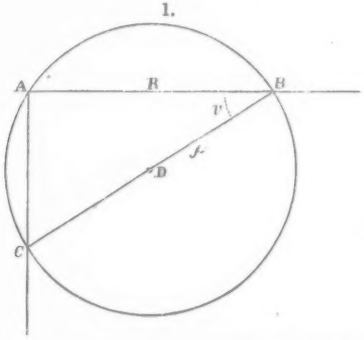


Fig. 1 represents this case; A is the slit, C is the eye-piece, and B is the grating with its center of curvature at C. In this case all the conditions are satisfied by fixing the grating and eye-piece to the bar BC whose ends rest on carriages moving on the rails AB and AC at right angles to each other; when desired, the radius AD may be put in to hold everything steady, but this has been found practically unnecessary.

The proper formulæ for this case are as follows. If λ is the wave-length and w the distance apart of the lines of the grating from center to center, then we have

$$\frac{1}{C} = \frac{\lambda N}{2w} = \frac{\sin \nu}{2}$$

where N is the order of the spectrum.

$$\therefore \lambda = \frac{w \sin \nu}{N}.$$

Now in the given case ρ is constant and so $N\lambda$ is proportional to the line AC. Or, for any given spectrum, the wave-length is proportional to that line.

If a micrometer is fixed at C we can consider the case as follows:

$$\frac{1}{C}=\frac{\lambda N}{2w}=\frac{1}{2}(\sin\mu+\sin\nu)$$

$$\frac{d\lambda}{d\mu}=\frac{w}{N}\cos\mu.$$

If D is the distance the cross-hairs of the micrometer move forward for one division of the head, we can write for the point C

$$d\mu=\frac{D}{\rho}$$

and for the same point μ is zero. Hence

$$\Delta\lambda=\frac{wD}{N\rho}.$$

But this is independent of ν and we thus arrive at the important fact that the value of a division of the micrometer is always the same for the same spectrum and can always be determined with sufficient accuracy from the dimensions of the apparatus and number of lines on the grating, as well as by observation of the spectrum.

Furthermore, this proves that the spectrum is normal at this point and to the same scale in the same spectrum. Hence we have only to photograph the spectrum to obtain the normal spectrum and a centimeter for any of the photographs always represents the same increase of wave-length.

It is to be specially noted that this theorem is *rigidly true* whether the adjustments are correct or not, provided only that the micrometer is on the line drawn perpendicularly from the center of the grating, even if it is not at the center of curvature.

As the radius of curvature of concave gratings is usually great, the distance through which the spectrum remains practically normal is very great. In the instrument which I principally use, the radius of curvature ρ, is about 21 feet 4 inches, the width of the ruling being about 5·5 inch. In such an instrument the spectrum thrown on a flat plate is normal within about 1 part in 1,000,000, for 6 inches and less than 1 in 35,000, for 18 inches. In photographing the spectrum on a flat plate, the definition is excellent for 12 inches, and by use of a plate bent to 11 feet radius, a plate of 20 inches in length is in perfect focus and the spectrum still so nearly normal as to have its error neglected for most purposes.

Another important property of the concave grating is that all the superimposed spectra are in focus at the same point,

and so by micrometric measurements the relative wave-lengths are readily determined. Hence, knowing the absolute wave-length of one line, the whole spectrum can be measured. Professor Peirce has determined the absolute wave-length of one line with great care and I am now measuring the coincidences. This method is greatly more accurate than any hitherto known, as by a mere eye inspection, the relative wave-length can often be judged to 1 part in 20,000 and with a micrometer to 1 in 1,000,000. Again, in dealing with the invisible portion of the spectrum, the focus can be obtained by examining the superimposed spectrum. Captain Abney, by using a concave mirror in the place of telescopes, has been enabled to use this method for obtaining the focus in photographing the ultra red rays of the spectrum. It is also to be noted that this theorem of the normal spectrum applies also to the flat grating used with telescopes and to either reflecting or transmitting gratings; but in these cases only a small portion of the spectrum can be used as no lens can be made perfectly achromatic. And so, as the distance of the micrometer has constantly to be changed when one passes along the spectrum, its constant does not remain constant but varies in an irregular manner. But it would be possible to fix the grating, one objective and the camera rigidly on a bar, and then focus by moving the slit or the other objective. In this case the spectrum would be rigidly normal, but would probably be in focus for only a small length and the adjustment of the focus would not be automatic.

But nothing can exceed the beauty and simplicity of the concave grating when mounted on a movable bar such as I have described and illustrated in Fig. 1. Having selected the grating which we wish to use, we mount it in its plate-holder and put the proper collimating eye-piece in place. We then carefully adjust the focus by altering the length of D until the cross-hairs are at the exact center of curvature of the grating. On moving the bar the whole series of spectra are then in exact focus, and the value of a division of the micrometer is a known quantity for that particular grating. The wooden way AC, on which the carriage moves is graduated to equal divisions representing wave-lengths, since the wave-length is proportional to the distance AC. We can thus set the instrument to any particular wave-length we may wish to study, or even determine the wave-length to at least one part in five thousand by a simple reading. By having a variety of scales, one for each spectrum, we can immediately see what lines are superimposed on each other and identify them accordingly when we are measuring their relative wave-length. On now replacing the eye-piece by a camera, we are in a position to photograph the spectrum with the greatest ease. We put in the sensitive plate, either

wet or dry, and move to the part we wish to photograph; having exposed for that part, we move to another part, raise the plate to another position and expose once more. We have no thought for the focus, for that remains perfect, but simply refer to the table giving the proper exposure for that portion of the spectrum and so have a perfect plate. Thus we can photograph the whole spectrum on one plate in a few minutes, from the F line to the extreme violet in several strips, each 20 inches long. Or we may photograph to the red rays by prolonged exposure. Thus the work of days with any other apparatus becomes the work of hours with this. Furthermore each plate is to scale, an inch on any one of the strips representing *exactly* so much difference of wave-length. The scale of the different orders of spectra are *exactly* proportional to the order. Of course the superposition of the spectra gives the relative wave-length. To get the superposition, of course, photography is the best method.

Having so far obtained only the first approximation to the theory of the concave grating, let us now proceed to a second one. The dividing engine rules equal spaces along the chord of the circular arc of the grating: the question is whether any other kind of ruling would be better. For the dividing engine is so constructed that one might readily change it to rule slightly different from equal spaces.

The condition for theoretical perfection is that C shall remain constant for all portions of the mirror. I shall therefore investigate how nearly this is true.

Let ρ be the radius of curvature and let R and r be the true distances to any point of the greating, $R_\circ$ and $r_\circ$ being the distances to the center. Let μ and ν be the general values of the angles and $\mu_\circ$ and $\nu_\circ$ the angles referred to the center of the mirror. The condition is that

$$\frac{2}{C}=\sin\mu+\sin\nu$$

shall be a constant for all parts of the surface of the grating. Let us then develop $\sin\mu$ and $\sin\nu$ in terms of $\mu_\circ$, $\nu_\circ$ and the angle δ between the radii drawn to the center of the grating and to the point under consideration. Let δ' be the angle between R and $R_\circ$. Then we can write immediately

$$\rho\sin\mu=\rho\sin\mu_\circ\cos\delta'+R_\circ\sin\delta'-\rho\cos\mu_\circ\sin\delta'$$

$$\sin\mu=\sin\mu_\circ\cos\delta'\left\{1+\frac{R_\circ}{\rho\sin\mu_\circ}A\tan\delta'\right\}$$

where

$$A=1-\frac{\rho\cos\mu_\circ}{R_\circ}.$$

Developing the value of $\cos\delta'$ in terms of δ, we have

$$\cos\delta'=\cos\delta\left\{1+\frac{A}{2}\left[1+\frac{\rho\cos\mu_\circ}{R_\circ}\right]\delta^2-\frac{\rho\sin\mu_\circ}{2R_\circ}\left[1+A\left(1+\frac{2\rho}{R_\circ}\right)\right]\delta^3+\&c.\right\}$$

As the cases we are to consider are those where A is small, it will be sufficient to write

$$\tan\delta'=\frac{\rho\cos\mu_\circ}{R_\circ}\delta.$$

Whence we have

$$\sin\mu=\sin\mu_\circ\cos\delta\left\{1+\cot\mu_\circ A\delta+\frac{A}{2}\left[1+\frac{\rho\cos\mu_\circ}{R_\circ}\right]\delta^2+\left[A^2\cot\mu_\circ\left(1+\frac{\rho\cos\mu_\circ}{R_\circ}\right)-\frac{\rho\sin\mu_\circ}{2R_\circ}\left(1+A\left(1+\frac{2\rho}{R_\circ}\right)\right]\delta^3+\&c.\right\}$$

We can write the value of $\sin\nu$ from symmetry. But we have

$$2\frac{db}{ds}=\sin\mu+\sin\nu.$$

In this formula, db can be considered as a constant depending on the wave-length of light, etc., and ds as the width apart of the lines on the grating. The dividing engine rules lines on the curved surface according to the formula

$$2\frac{db}{ds}=\cos\delta(\sin\mu_\circ+\sin\nu_\circ).$$

But this is the second approximation to the true theoretical ruling. And this ruling will not only be approximately correct, but exact when all the terms of the series except the first vanish. In the case where the slit and focus are on the circle of radius $\frac{1}{2}\rho$, as in the automatic arrangement described above, we have $A=0$ and the second and third terms of the series disappear, and we can write since we have

$$\frac{R_\circ}{\rho}=\cos\mu_\circ \text{ and } \frac{r_\circ}{\rho}=\cos\nu_\circ$$

$$2\frac{db}{ds}=\cos\delta(\sin\mu_\circ+\sin\nu_\circ)\left(1-\frac{1}{2}\frac{\sin\mu_\circ\tan\mu_\circ+\sin\nu_\circ\tan\nu_\circ}{\sin\mu_\circ+\sin\nu_\circ}\delta^2+\&c.\right)$$

But in the automatic arrangement we also have $\nu_\circ=0$, and so the formula becomes

$$2\frac{db}{ds}=\cos\delta(\sin\mu_\circ+\sin\nu_\circ)\left\{1-\frac{1}{2}\tan\mu_\circ\delta^2+\&c.\right\}$$

To find the greatest departure from theoretical perfection, δ must refer to the edge of the grating. In the gratings which

I am now making, ρ is about 260 inches and the width of the grating about 5·4 inches. Hence $\delta = \frac{1}{100}$ approximately and the series becomes

$$1 - \frac{1}{2{,}000{,}000} \tan \mu_0.$$

Hence the *greatest* departure from the theoretical ruling, even when $\tan \mu_0 = 2$, is 1 in 1,000,000. Now the distance apart of the components of the 1474 line is somewhat nearly one forty-thousandth of the wave-length and I scarcely suppose that any line has been divided by the best spectroscope in the world whose components are less than one-third of this distance apart. Hence we see that the departure of the ruling from theoretical perfection is of little consequence until we are able to divide lines twenty times as fine as the 1474 line. Even in that case, since the error of ruling varies as δ^3, the greater portion of the grating would be ruled correctly.

The question now comes up as to whether there is any limit to the resolving power of a spectroscope. This evidently depends upon the magnifying power and the apparent width of the lines. The magnifying power can be varied at pleasure and so we have only to consider the width of the lines of the spectrum. The width of the line evidently depends, in a perfect grating, upon three circumstances, the width of the slit, the number of lines in the grating and the *true physical width of the line.* The width of the slit can be varied at pleasure, the number of lines on the grating can be made very great (160,000 in one of mine), and hence we are only limited by the true physical width of the lines. We have numerous cases of wide lines, such as the C line, the components of the D* and H lines and numerous others which are perfectly familiar to every spectroscopist. Hence we are free to suppose that all lines have some physical width, and we are limited by that width in the resolving power of our spectroscope. Indeed, from a theoretical standpoint, we should suppose this to be true: for the molecules only vibrate freely while swinging through their free path and in order to have the physical width one one-hundred-thousandth of the wave-length, the molecule must make somewhat nearly one hundred thousand vibrations in its free path: but this would require a free path of about two inches! Hence it would be only the outermost solar atmosphere that could produce such fine lines and we could hardly expect to see much finer ones in the solar

* I have recently discovered that each component of the D line is double probably from the partial reversal of the line as we nearly always see it in the flame spectrum.

spectrum. Again* it is found impossible to obtain interference between two rays whose paths differ by much more than 50,000 wave-lengths.

All the methods of determining the limits seem to point to about the 150,000th of the wave-length as the smallest distance at which the two lines can be separated in the solar spectrum by even a spectroscope of infinite power. As we can now nearly approach this limit I am strongly of the opinion that we have nearly reached the limit of resolving power, and that we can never hope to see very many more lines in the spectrum than can be seen at present, either by means of prisms or gratings.

It is not to be supposed, however, that the average wave-length of the line is not more definite than this, for we can easily point the cross hairs to the center of the line to perhaps 1 in 1,000,000 of the wave-length. The most exact method of detecting the coincidences of a line of a metal with one in the solar spectrum would thus be to take micrometric measurements first on one and then on the other; but I suppose it would take several readings to make the determination to 1 in 1,000,000.

Since writing the above I have greatly improved my apparatus and can now photograph 150 lines between the H and K lines, including many whose wave-length does not differ more than 1 in about 80,000. I have also photographed the 1474 and b_3 and b_4, widely double, and also E just perceptibly double. With the eye much more can be seen, but I must say that I have not yet seen many signs of reaching a limit. The lines yet appear as fine and sharp as with a lower power. If my grating is assumed to be perfect, in the third spectrum I should be able to divide lines whose wave-lengths differed, in about 150,000, though not to photograph them.

The E line has components, about $\frac{1}{60000}$th of the wave-length apart. I believe I can resolve lines much closer than this, say 1 in 100,000 at least. Hence the idea of a limit has not yet been proved.

However as some of the lines of the spectrum are much wider than others we should not expect any *definite* limit, but a gradual falling off as we increase our power. At first, in the short wave-lengths at least, the number of lines is nearly proportional to the resolving power, but this law should fail as we approached the limit.

* This method of determining the limit has been suggested to me by Prof. C. S. Hastings, of this University.

ART. XII.—*Glacial Markings of Unusual Forms in the Laurentian Hills;** by EDMUND ANDREWS, M.D., LL.D.

TWO summer vacations spent in camps and canoes where the Laurentian Hills skirt the northeast shore of Lake Huron have brought to my notice some glacial phenomena of very unusual forms.

These hills are for the most part ranges of hard metamorphic rock, which, at the bowlder drift period, were scraped bare and washed clean of all earthy covering, so that the present forests grow mostly in a thin stratum of vegetable mold of later origin. The irregular and knobby eminences, as the country slopes gradually beneath the level of Lake Huron, project their countless summits above the water in a wide belt of wooded islands, which extend along the coast some two hundred and fifty miles. The British chart (Bayfield's original) marked fifty-two thousand of these islands. The rocks along the coast are mostly white quartzite and gneiss, and are everywhere covered with glacial markings, which are often of peculiar forms. North and northeast of Grand Manitoulin Island, the Cloche Mountains stretch east and west about thirty miles along the coast. These mountains are of white quartzite and the strata are nearly perpendicular, with their striæ parallel to the range, that is, east and west. They are covered everywhere with striations, which, owing to the intense hardness of the material, retain their forms with beautiful distinctness.

CURVED STRIÆ.

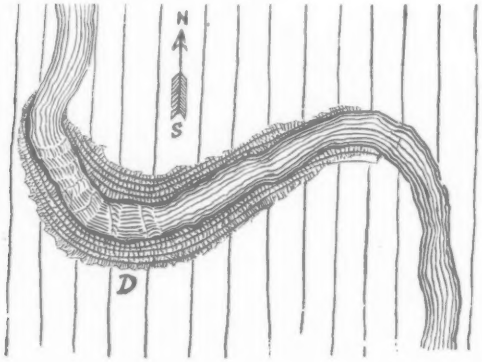
Among the interesting phenomena of the region are the thousands of curved striations. Fig. 1 is from the stream that comes down by the Hudson's Bay Post, about fifteen rods above the cascade. There is a broad gap here in the Cloche Range through which some fine lakes, lying behind the mountains, send their surplus waters to Lake Huron. The stream apparently ran in the same bed before the drift period, for the little gorge at D (indicated in the figure by shading), scarcely ten feet deep, is well scored to the very bottom with glacial striæ following all the sinuosities of the stream. The general course of the striæ of the gap is southward. The perpendicular lines in the figure indicate this general direction, while the curved lines in the shading of the gorge D show how the striæ bend abruptly in order to follow around the hard quartzite banks of the stream, and in doing so even run a little contrary to the main course of the glaciation.

The crest of the Cloche Mountains is crossed by a multitude

* Read before the Chicago Academy of Sciences.

of giant grooves, some of which reach a depth of six feet, and are twenty feet across. These markings run southward, round-

1.



ing slightly over the summit of the range and down its slopes, until they reach the crests of its southern precipices where they terminate abruptly, as it were, sailing away into the air and not forming any grooves down its face.

Sixty miles southeast of the Cloche gap and off the mouth of French River, there lies, outside of the general insular belt, a beautiful cluster of wooded islets, the Bustard Isles. The group consists of about two hundred great *roches moutonnées*, upon which sufficient vegetable mold has accumulated to support a thick growth of trees. Wherever this soil has been washed away by the waves the striæ come to view.

2.

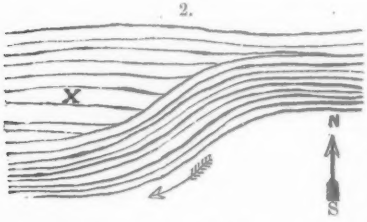
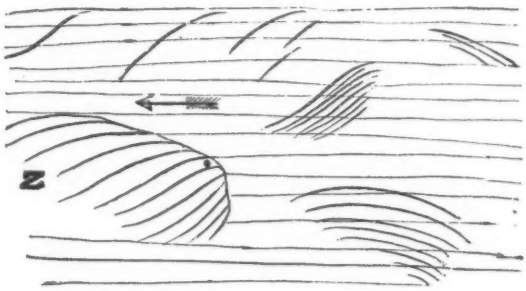


Fig. 2 was sketched from a sample of curved markings near one of my camps on the north side of the group. The sketch represents about fifteen feet of the length of the marks. It is not easy to see any reason for the curve, as there was no prominence of rock in the direction of X to turn the ice. In fact the islet was highest on the side toward which the ice turned at the first curve. The compass mark is approximately, but not precisely correct.

Fig. 3 is copied from my notes of observation on striæ found at Negaunee, in northern Michigan. A knob of rock uncovered by iron miners was of such material that it showed on its irregular surface the finest markings, even to hair lines. There

were upon this rock some curves which were evidently deflections caused by knobs and bosses on its surface, as for instance

3.

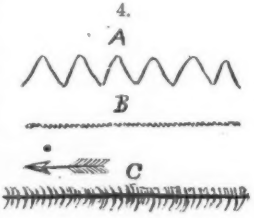


at Z. While other markings were erratic and curved without obvious cause, as though the ice had been swayed by swirling currents as the waters moved about it. The most of the glaciation was in the direction shown by the horizontal lines. The curved lines in the figure were selected from hundreds of others on a surface of about two rods square. They were generally short, and some of the curves were of less than one foot radius.

Serrated Striæ.

Behind the Cloche Mountains the Spanish River runs westward into Lake Huron. A branch of this stream, called the Sable, coming down from the hills on the north, presents near its mouth five cataracts within a distance of eight miles. At the lowest of the falls the river runs through a sort of rock flume, having upon both sides walls about forty feet in height, not quite vertical but with a slight inclination away from the stream. These cliffs are smooth and striated in every part parallel to the stream. At the falls, which are only a few feet in height, the striæ curve with the descent, and also laterally with a bend of the cliff. On the walls of the gorge are to be seen a few examples of the marks A and B. Fig. 4 and the mark A is serrated, the serrations being perhaps twelve inches high. It is not easy to explain the cause of these striæ in a perfectly satisfactory manner, but it would seem that ice must have been driven through the flume with a rocking motion so that the bowlders on its lateral margins were caused to take a zigzag course, scoring the walls in a corresponding form. In B, fig. 4,

4.



is represented a section of certain marks produced in the same manner as those cuts of a stone planing engine where the tool trembles or vibrates in the grasp of the machine so as to cut a finely serrated groove. In the specimens found the serrations were about one-quarter of a centimeter from crest to crest. It is possible that the regular vibrations thus recorded on the rocks had some fixed mathematical relation to the velocity of the ice, which might possibly be determined by calculation or experiment.

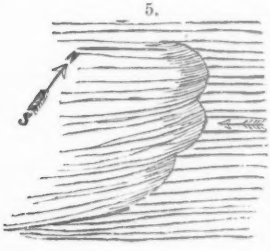
New Index of the Direction of Motion.

The mark C, fig. 4, was copied from a granitic *roche moutoneé* at the fishing hamlet of Killarney near some quartzite ranges named "Killarney Mountains." Striæ of this type show a multitude of minute cracks extending laterally and curved so as to present their points forward in the direction of the glacial movements. Only large grooves and scratches show this peculiarity. It is a very convenient mode of determining the direction of the motion, for the pressure of the bowlders which made the scratches as they moved forward caused the transverse cracks to present their concave sides always forward. It is well known to engineers that in brittle substances the forms assumed by fractures vary with the velocity of the impact, so that there is probably a time relation involved here also which would make it possible to determine the approximate velocity of the ice which carried the bowlders. Such a calculation, if based upon carefully repeated experiments, might prove an important contribution to our scanty knowledge of the Drift Period.

Scoop Marks.

These are singular phenomena and very difficult of explanation. They are of two varieties, the striated and the unstriated. Fig. 5 is a diagram intended to illustrate a typical form of the striated variety. The marks consist of shallow elongated excavations or troughs which may be many feet in width. They run nearly in the direction of the general striation of the locality, and look as though a huge flour scoop had been inserted into the rock and had cut out enough of its substance to make a smooth and rather shallow concave channel. The end toward the east, or northeast from whence the drift action came, is abrupt, sharply-defined, and although the angle of junction with

5.



the general surface is obtuse, yet this line of junction is a sharp and clearly cut edge and not a gradual sloping or rounding into the general level. The interior of the concavity is well striated in the direction of its own length, but the scratches are never continuous with those of the general surface northeast of the head of the scoop mark; and, conversely, the striæ of the general glaciated surface are all cut off abruptly at the head of the scoop and never descend at all into it.

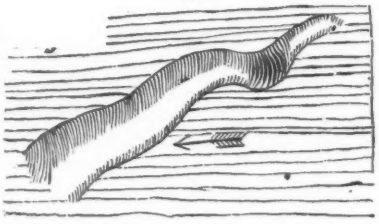
The tail of the channel grows shallower as it runs west or southwestward, and its thin extremity merges imperceptibly into the glaciated surrounding surface. A few of the scoop-marks are nearly flat in contour and are only recognizable where they cut abruptly through at an obtuse angle into the general face of the rock.

The interpretation of these anomalous scoop-marks is somewhat perplexing; at least, after considerable study of them, I am quite unable to frame a plausible theory of their origin other than the general one, that ice was the agent.

The unstriated scoop-marks differ considerably from the others in form, and on the average are smaller, seldom exceeding two feet in breadth. There are no glacial markings in their smooth but unpolished concavities.

Whether they never contained any striations or whether the markings have been erased by sand and water driven through them before the Drift Period closed is not easily determined. These channels generally are crooked without obvious reason (see fig. 6), and are narrowest at the end toward the northeast, from which direction the drift came. Toward the opposite extremity they grow wider and more shallow, disappearing finally and vaguely into the general ice-marked surface of the locality. The specimen shown in fig. 6 is perhaps six feet long, sunk in granite, without any crevice-vein or visible irregularity in the rock face to account for its excavation at that spot. These channels often differ pretty widely in direction from the adjacent striation, and the end nearest the northeast commences as a rounded depression without the clear cut edge presented by the striated scoop-marks. The darkly shaded spot in fig. 6 was deeper than the rest, as if from a species of "pot-hole" action.

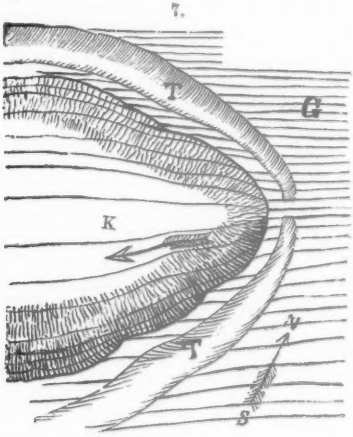
6.



Most of the striated scoop-marks are not independent phenomena, like fig. 6, but evidently are appendages to adjacent knobs and projections of rock. The type of such cases is rep-

resented in fig. 7, which is a diagram of the plan of numerous specimens seen along the eastern shore from Killarney to Parry Sound, a distance of about one hundred miles. K is a boss or knob of rock projecting above the general glaciated surface in an oval form —in fact, a *roche moutonneé.* The specimen in fig. 7 is about four feet high, fifteen wide and thirty long; but all sizes and irregular forms are common. The observed specimens are mostly in gneiss. The horizontal lines in the diagram show the general direction of the striation in the locality — nearly west-southwest, as shown by the arrow. It will be seen that as the striæ approach and rise upon the surface of the knob they are deflected to the right and left and sweep over it in an oblique course. This sort of curved deflection, partly over and partly around obstacles is common to the whole coast, so that in many places almost all the striæ are curved by the influence of the knobby surfaces of the gneiss and quartzite.

7.



T T are two unstriated scoop-marks having a length of about ten feet and a width of twelve inches. They begin vaguely near to each other, but not in contact, close to the northeast end of the knob, and rapidly deepen to six inches or more as they curve about its two sides, after which they grow wider and more shallow until they become lost in the general glaciation along the sides of the rocky buttress. There are no striæ in these concavities and their inner borders do not touch the base of the knob but keep a foot or more away from it, leaving a narrow strip of level striated rock between the margins of the troughs and the eminence.

In a multitude of cases like this it is sufficiently clear that the resistance offered by knobs of rocks to the progress of the drift agencies in some way determined the presence and direction of these scoop-marks.

The great belt of fifty-two thousand islands, above referred to, varies from three to fifteen miles in width and is about two hundred and fifty miles long. Beginning at the St. Marie River it first outlines the north channel by the great Manitoulin group, and thence, passing southeast through Frazer Bay, continues along the whole east coast of Georgian Bay and termi-

nates near Collingwood. Near the main land the islands are mostly metamorphic with a very distorted stratification, but a few of those on the lakeward border of the belt are of Silurian limestone with the strata dipping gently away from the nearest metamorphic hills.

This almost untrodden solitude, which has lain forgotten by the crowds of summer pleasure seekers, is well worthy of a visit by the lover of nature. The magnificent panoramas of the island-belt as viewed from the summits of the La Cloche and Killarney ranges are unique and in themselves well worth the journey to the region. Fortunately they are as yet almost unknown to sight-seers and still remain in their original freshness and silence.

ART. XIII.—*Response to the Remarks of Messrs. Wachsmuth and Springer on the genera Glyptocrinus and Reteocrinus;* by S. A. MILLER.

IN response to the remarks of the distinguished paleontologists, Messrs. Wachsmuth and Springer, on the genera *Glyptocrinus* and *Reteocrinus*, in the April No. of this Journal, p. 255, I would say, that the first issue joined, between us, is one of law and not of fact and may be stated as follows:

In 1858, Can. Org. Rem., Decade 4, p. 63, Prof. Billings defined the genus *Reteocrinus* and the species *Reteocrinus stellaris* from the Trenton Group, at Ottawa, Canada, and also, with much doubt referred another species to the same genus. In 1866, in advance sheets of the 24th Rep. N. Y. St. Mus. Nat. Hist. p. 206, Prof. Hall defined the species *Glyptocrinus Nealli*, from the upper part of the Hudson River Group, at Lebanon, Ohio. In 1881. Revision of the Palæocrinoidea, pt. 2, p. 191, Messrs. Wachsmuth and Springer reconstructed the genus *Reteocrinus*, with *Glyptocrinus Nealli* as the type, and, in their remarks above referred to (p. 264), they defend their right to retain a generic name and substitute any species as the type of the genus (always of course having a good specimen from which to ascertain the characters). If, they say, "It happens, that the true characters of the group are better and more comprehensively expressed in some other species than the one first described, there is, in our opinion, not the least objection to adopting it as the type of the genus thus rectified." They think their practice, in this respect, fully justified, and aver they intend to adhere to it, and say they find "other good authorities do the same thing."

The question is, Can a subsequent author revise a genus and

substitute as its type a species unknown to the original author of the generic name, or not included by him, among his types of the genus, or not, itself, originally made or intended to be made the type?

I deny that the subsequent author has any such privilege, under the laws and rules of science, and affirm that no matter how learned the naturalist, how eminent the scientist, or inherently able and accurate the diagnosis may be, such work is an absolute nullity.

The issue is then, as before remarked, one of law, and the importance of determining it is not confined, in its scope, to the genera under consideration, nor to paleontology, but extends throughout Natural History. If I am right and should be able to convince Messrs. Wachsmuth and Springer, that such is the case, there is no doubt, they would, notwithstanding previous opinions, follow the law, because they are not only learned paleontologists but devotees of science.

I will quote from the rules for rendering the nomenclature of Zoology uniform and permanent adopted at the 12th meeting of the British Association for the Advancement of Science in 1842.

"It being admitted on all hands that words are only the conventional signs of ideas, it is evident that language can only attain its end effectually by being permanently established and generally recognized. This consideration ought, it would seem, to have checked those who are continually attempting to subvert the established language of Zoology by substituting terms of their own coinage. But, forgetting the true value of language, they persist in confounding the *name* of a species or group with its *definition;* and because the former always falls short of the fullness of expresion found in the latter, they cancel it without hesitation, and introduce some new term which appears to them more characteristic, but which is utterly unknown to the science, and is, therefore, devoid of all authority. If those persons were to object to such names of men as *Long*, *Little*, *Armstrong Golightly*, etc., in cases where they fail to apply to the individuals who bear them, or should complain of the names *Gough*, *Lawrence*, or *Harvey*, that they were devoid of meaning, and should hence propose to change them for more characteristic appellations, they would not act more unphilosophically or inconsiderately than they do in the case before us; for, in truth, it matters not, in the least, by what conventional sound we agree to designate an individual object, provided the sign to be employed be stamped with such an authority as will suffice to make it pass current. Now, in Zoology, no one person can subsequently claim an authority equal to that possessed by the person who is the first to define a new genus or describe a new species; and hence it is, that the name originally given, even though it may be inferior in

point of elegance or expressiveness to those subsequently proposed, ought as a general principle to be permanently retained. To this consideration we ought to add, the injustice of erasing the name originally selected by the person to whose labors we owe our first knowledge of the object; and we should reflect how much the permission of such a practice opens a door to obscure pretenders for dragging themselves into notice at the expense of original observers."

"The name originally given by the founder of a group, or the describer of a species, should be permanently retained to the exclusion of all subsequent synonyms."

"As the number of known species which form the groundwork of zoological science is always increasing, and our knowledge of their structure becomes more complete, fresh generalizations continually occur to the naturalist, and the number of genera and other groups requiring appellations is ever becoming more extensive. It thus becomes necessary to subdivide the contents of old groups, and to make their definitions continually more restricted. In carrying out this process, it is an act of justice to the original author that his generic name should never be lost sight of; and it is no less essential to the welfare of the science, that all which is sound in its nomenclature should remain unaltered amid the additions which are continually being made to it."

"A generic name, when once established, should never be cancelled in any subsequent subdivision of the group, but retained in a restricted sense for one of the constituent portions."

"When a genus is subdivided into other genera, the original name should be retained for that portion of it which exhibits in the greatest degree its essential characters as at first defined. Authors frequently indicate this by selecting some one species as a fixed point of reference, which they term, the 'type of the genus.' When they omit doing so, it may still in many cases be correctly inferred that the *first* species mentioned on their list, if found accurately to agree with their definition, was regarded by them as the type. A specific name or its synonyms will also often serve to point out the particular species, which by implication must be regarded as the original type of a genus. In such cases we are justified in restoring the name of the old genus to its typical signification, even when later authors have done otherwise."

"The generic name should always be retained for that portion of the original genus which was considered typical by the author."

"*Example.*—The genus *Picumnus* was established by Temminck, and included two groups, one with four toes, the other with three, the *former* of which was regarded by the author as typical. Swainson, however, in raising these groups at a later period to the rank of genera, gave a new name, *Asthenurus*, to the *former* group, and retained *Picumnus* for the *latter*. In this case we have no choice but to restore the name *Picumnus* Tem., to its correct sense, canceling the name *Asthenurus* Sw.,

and imposing a new name on the three-toed group which Swainson had called *Picumnus*."

"When no type is indicated, then the original name is to be kept for that subsequent subdivision which first received it."

"When the evidence as to the original type of a genus is not perfectly clear and indisputable, then the person who first subdivides the genus may affix the original name to any portion of it at his discretion, and no later author has a right to transfer that name to any part of the original genus."

"When an author infringes the law of priority, by giving a new name to a genus which has been properly defined and named already, the only penalty which can be attached to this act of negligence or injustice is to expel the name so introduced from the pale of science."

"When two authors define and name the same genus, both making it exactly of the same extent, the later name should be canceled *in toto*, and not retained in a modified sense."

"No special rule is required for the cases in which the later of two generic names is so defined as to be *less extensive* in signification than the earlier, for if the later includes the type of the earlier genus, it would be canceled by the operation of the rule that the generic name should always be retained for that portion of the original genus which was considered typical by the author."

"If the later name be so defined as to be equal in extent to two or more previously published genera, it must be canceled, *in toto*."

"A genus compounded of two or more previously proposed genera, whose characters are now deemed insufficient, should retain the name of one of them. If these original generic names differ in date, the oldest one should be the one adopted."

The committee on zoological nomenclature, consisting of Wm. H. Dall, who was assisted by Dr. Asa Gray, appointed by the American Association for the Advancement of Science in 1876, reported to the Nashville meeting in 1877. Upon the subject of names to be preserved in writing, dividing or modifying the limits of existing groups, the committee said:

"§ L. A change in the diagnostic characters, or a revision which carries with it the exclusion of certain elements of a group, or the inclusion of new elements, does not authorize the change of the name or names of a group."

"§ LI. When a group or genus is divided into two or more groups, the original name must be preserved and given to one of the principal divisions. The division including the typical species of the primitive genus, if any type had been specified, or the oldest, best known or most characteristic of the species originally included when the primitive genus was first described by its author, is the portion for which the original name is to be preserved. If there is no section specially so distinguished, that

which retains the larger number of species should retain the old name (DC.), but the latter cannot be applied to a restricted group containing none of the species referred to the primitive group by its author at the time when it was described or when he enumerated the species contained in it."

In the preparation of this report the author placed great dependence on the memoir of DeCandolle, and presented to the working naturalists a series of questions, requesting answers, which were quite fully responded to. The 12th question and the answers were as follows:

"Shall a subsequent author be permitted in revising a composite genus (of which no type was specified when it was described) to name as its type a species not included by the original author of the genus in that latter author's list of species given when the genus was originally described? No 37. Doubtful 2. Yes 5. No answer 1."

I will cite only one more authority. Professor Meek in discussing the type of the genus Straparollus says:

"It seems to us, however, that if the name *Euomphalus* is to be retained at all, we should apply it to the forms for which it was originally proposed, and that we have no right to transfer it to another type, because Sowerby subsequently, in another place, refers this other type to his genus *Euomphalus*."—Geol. Surv. Ill., vol. ii, p. 158.

The rule that a subsequent author cannot revise a genus and substitute as its type a species different from that relied upon by the founder of the genus seems to be as well settled in England and America as any other rule or law upon the subject of nomenclature in Natural History, and I know of no author of recognized merit in either country, save Messrs. Wachsmuth and Springer, who has attempted to violate it. The subject is not new, and the instances of strictly adhering to the rule, under circumstances where it would have seemed to accommodate the author to violate it, are numerous. For instance, Professor Hall, mistaking the type of the genus *Retzia*, proposed and defined the genus *Rhynchospira;* afterward ascertaining that *Rhynchospira* was a synonym for *Retzia*, he abandoned it and proposed *Rhynchotreta* for the form which he had originally mistaken for *Retzia*. Had it not been for this rule he might have abandoned *Retzia evax* as the type of his genus *Rhynchospira*, and substituted *Rhynchonella cuneata*, which became the type of *Rhynchotreta*. You will look in vain in the works of such authors as Hall, Billings, Cope and Meek for any support of the views of Wachsmuth and Springer upon this subject; and the reason is obvious. If they can substitute another than the original species as the type of a genus, I can substitute yet another, and you can another, and so we destroy all *fixity* in

the type and designated characters, throw the science into confusion, and seriously impair the value and reliability of generic characters, besides losing all interest in the original work of the author who established the genus.

The conclusion is inevitable that, as a matter of law or rule of science, *Glyptocrinus Nealli* cannot be made the type of the genus *Reteocrinus*, and the diagnosis must be canceled and stricken from the pale of science.

Nor are we in accord in respect to the value of the genus *Reteocrinus*, nor as to the relationship existing between it and *Glyptocrinus Nealli*, for I regard the latter as having a closer affinity to *Glyptocrinus decadactylus* than it has to *Reteocrinus stellaris*, though it may fairly be regarded as holding an intermediate position. I have had the pleasure of seeing the original specimen represented by fig. 4*a*, pl. 9, of Decade 4, which is the type of the genus *Reteocrinus*, and from recollection think the figure is a correct representation of it. Now let us compare it with *G. Nealli;* of course the comparison can only be made with the azygous side and the column.

G. Nealli.—Column sharply pentagonal and composed of alternating thin and thicker pieces.

R. stellaris.—Column round and composed of very thin plates.

G. Nealli.—Basal plates very small, presenting a low triangular face on the exterior (though minutely truncated at the lateral angles) and not interfering with the pentalobate character, as viewed from below, in following the depressions in the column across the central part and greatest height of the basals and beyond the lower lateral sides of the subradials.

R. stellaris.—Basals large, presenting an hexagonal face on the exterior and bearing a strong bow-shaped ridge with sinus or concave side upward, not in contact with the margins, except where it meets corresponding ridges on the succeeding plates above; the remaining portions of these plates depressed from $\frac{1}{2}$ to $\frac{3}{4}$ of a line. No pentalobate character in the central part of these plates, but deep depressions at their lower lateral sides.

G. Nealli.—Subradials about as wide as long, except the one on the azygous side which is a little longer than wide, and each bearing a semicylindrical three-rayed ridge, highest in the central part and sending one arm below to meet the angles of the column and one to each of the adjoining radials to meet corresponding semicylindrical ridges, except the subradial, on the azygous side, which bears an additional depressed semicylindrical ridge extending upward to the superior truncated side.

R. stellaris.—Subradials very large, longer than wide, and each bearing a more than semicylindrical double-bifurcated or four-rayed ridge, two of the ridges on each uniting with the

concave ridges on the basals below and two uniting with similar ridges on the primary radials above. The subradial on the azygous side bears a fifth or additional ridge extending upward from the middle of the concave side of the upper bifurcating ridges to the superior truncated side or middle of the azygous interradius. The depressions between these plates or the height of these ridges is $\frac{2}{4}$ of a line.

G. Nealli.—Primary radials three in each series, except in the left posterior ray, which has only two. These plates are slightly longer than wide, the first and third, or in the left posterior ray the first and second, are pentagonal and of almost exactly the same size, form and ornamentation, so that by reversing ends they would almost fill each other's places. The second plate in four series is subquadrangular.

R. stellaris.—Primary radials four in each series, the first one very large, bearing a high bifurcated ridge which unites with corresponding ridges on the subradials on either side below; the second much smaller and shorter; the third diminished in size a little more; and the fourth very small in comparison with the first, evidently not more than one-fourth as large.

G. Nealli.—Secondary radials, twelve to about sixteen in each series, four or five of the lower ones being larger and proportionally longer, than those above (which are very short, free, and scarcely differ from the free arm pieces); the second or third one having somewhat the character of an axillary piece, and giving off a small lateral division or series of pieces on the outer or interradial side, that does not become free, but is soldered in with the interradials to the top of the body where it gives origin to a pinnule like those of the arms.

R. stellaris.—Secondary radials, four to six in each series very gradually diminishing in size, without any evidence of a lateral division from either plate.

G. Nealli.—Azygous interradial area covered by fifty or sixty plates, very unequal in size, the middle row being decidedly larger and more prominent than the others, so as to form a ridge up the middle, while the other smaller and less prominent ones are crowded in, irregularly, on each side. The plates in this middle row, however, have no uniformity in size or shape; the first one is large and elongated, the fourth is small and subquadrate; and the row has become about obsolete at the sixth plate, where all are of nearly the same size and scarcely distinguishable from the minute pieces which cover the flattened vault and with which they unite.

R. stellaris.—Azygous interradial area covered by a large number of plates, probably one hundred or more, very unequal in size, the middle row being decidedly larger and more prominent than the others so as to form a ridge up the middle. The

plates in this row, however, do not rapidly diminish in size and fade out in their distinctive character before reaching the top of the vault; on the contrary they are longer than the primary radials, four of them reach nearly as high as the last of the secondary radials, and while the specimen is not preserved above this, enough is disclosed to the paleontologist to show, that this series continued up the face of a proboscis that extended may be as far or farther than the arms and the pinnules.

If we look at the general aspect and form of *G. Nealli* and *R. stellaris* there is as little resemblance as we have found in the more particular comparison. *G. Nealli* has a pentalobate obconoidal calyx and large flowing arms and pinnules. *R. stellaris* has a saucer-shaped calyx below, it is elongated in the region of the primary radials and has small or diminutive arms and pinnules. Indeed there is no striking resemblance between the two species, except in the depressed interradial areas covered by numerous plates, but even this resemblance is not continued in the upward extension of the azygous interradial area.

Therefore, notwithstanding the learning and usually skillful judgment of Messrs. Wachsmuth and Springer in regard to fossils belonging to this class, fortified as it is by the opinion of my young friend Walter R. Billings and by Professor A. G. Wetherby, and the confidence with which these authors assert, that "The question of the generic identity of *G. Nealli* and allied forms with *Reteocrinus*, may be considered at rest," I believe, and express the opinion without hesitation, that they are *not* congeneric, and that *Reteocrinus stellaris* is so far removed from *Glyptocrinus Nealli* that it is doubtful whether they should even be classified in the same family.

Messrs. Wachsmuth and Springer call attention to the fact that in describing *Glyptocrinus Pattersoni* I said it differed from other species of *Glyptocrinus* in having only ten arms; there was a thoughtless omission on my part to except *Glyptocrinus Baeri;* but they say *Glyptocrinus Nealli* and *Glyptocrinus cognatus*, which I had described only a year before, have only ten arms. This is to me incomprehensible, for *Glyptocrinus Nealli* was described by Hall and again by Meek (Ohio Pal., vol. i, p. 35) as a species bearing twenty arms, and I have seen the type, and have examined more than a hundred specimens each having twenty arms and never saw a specimen without that number. As to *Glyptocrinus cognatus*, the number of arms was unknown, at the time the species was described, but I suppose it to have had either twenty or twenty-four. I have now before me three specimens so nearly allied to it that it is hard to point out specific differences. Each of them show the arms and each have twenty-four.

I do not desire to be understood as saying that it is either

inexpedient or improper to separate the species now arranged under *Glyptocrinus* and distribute them in two or three genera; no satisfactory work, however, has yet been done in that direction; and if *Glyptocrinus Nealli*, *G. Baeri*, *G. subglobosus* and *G. fimbriatus* are thrown in one section, then I am free to say it is better that all shall remain under the present generic name. Moreover, I think subgeneric names in paleontology ought not to be encouraged, because they are inconsistent with the binomial system of nomenclature and really serve no scientific purpose.

To review the species now classed with *Glyptocrinus* and point out their differences in structure would too greatly lengthen this article. It may be done in another communication.

ART. XIV.—*On the Present Status of the Eccentricity Theory of Glacial Climate;* by W. J. McGEE.

THE recent appearance of an important treatise, in which Croll's theory of secular variations in terrestrial climate is given a prominent place,[1] has elicited some adverse criticism of that theory, by different reviewers, which can only be regarded as embodying the current objections to the adoption of the eccentricity theory in general.

In addition to an indefinite general argument such as might equally be brought to bear against any intricate and comprehensive theory involving principles falling within the domains of diverse nascent branches of science, Gilbert[2] urges three definite and specific objections against the theory:

1. "If it is true, then epochs of cold must have occurred with considerable frequency through the entire period represented by the stratified rocks; and iceberg drift, if no other traces, should have been entombed at numerous horizons. It has not been found, however, and of the eight horizons claimed by Croll to show evidence of glacial action, the treatise under consideration mentions only two with confidence, and two others with doubt. In the two instances to which queries are not attached, the phenomena appear to indicate local and not general glaciation.

2. If the hypothesis is true, the cold of the Glacial epoch must have been many times interrupted by intervals of exceptional warmth; but little has been added to the evidence adduced by Croll for such an interruption, and in America, where there is now great activity in the investigation of glacial phenomena,

[1] "Text Book of Geology," A. Geikie, 1882, 21–29. [2] Nature, xxvii, 262.

the evidence of a *single* inter-glacial period is cumulative and overwhelming, while there is no indication whatever of more than one.

3. If the hypothesis is true, submergence in polar and temperate regions should have been coincident with glacial expansion, and emergence coincident with glacial retreat, but the Quaternary history of Great Britain, as drawn in the new text-book, includes two periods of maximum ice-extension, *separated* by a period of submergence."

The editor of the American Naturalist[3] insists 1, that the "hypothetical stoppage of the Gulf Stream to account for the glacial climate of Northern Europe is not warranted by paleontological facts," and 2, that,—"So extremely hypothetical, from paleontological considerations, is the evidence of the so-called 'interglacial periods,' * * that we wonder that our author should endorse Dr. Croll's speculations without stating some of the facts supposed to sustain such a view."

With the conservatism characteristic of British specialists, Lapworth,[4] in a seven-word clause, relegates the theory to the domain of "attractive speculation."

Now these criticisms, coming as they do from the most eminent sources, and appealing as they do to indisputable facts, carry exceptional weight, and go far toward determining the future status of the theory against which they are directed; and ignoring, as they do, all of the eccentricity theory except in so far as it was originally enunciated, and is now advocated by Dr. Croll, must naturally be regarded by general students as summarily disposing of the whole subject. It is therefore most desirable that their validity should be unquestioned, that the force and bearing of the several objections should be unmistakable, and that at least the combined reviews should present fairly the status of current opinion concerning the various phases of the entire theory to which they are apparently designed to apply. To the writer these conditions do not appear to be fulfilled; and since the eccentricity theory, as now embraced by numerous students, has been specially framed to meet the difficulties urged by the reviewers, it appears to him necessary that the failure of the criticisms should be impressed upon readers of current geological literature.

So long ago as 1878, LeConte[5] showed that if the cold of the Quaternary were the joint result of eccentricity, precession, and secular refrigeration, it may have culminated in glacial conditions but once. More recently the subject has been admirably discussed by Wallace, in a treatise which has not yet received adequate attention on this side of the Atlantic.[6] It is there

[3] xvii. 177–8.
[4] Geol. Mag., x, 80.
[5] "Elements of Geology," 1st ed., 549.
[6] "Island Life," 1881, Chs. VIII, IX.

established that, as long ago suggested by Lyell, continental configuration must exercise an important influence on the accumulation of land ice, and that the pre-Quaternary geography of the northern hemisphere was probably such as to preclude extensive ice-accumulation. In this view the recognized fundamental elements of glacial climate are eccentricity, precession and continental configuration (to which should be added, secular refrigeration). Such view has already been favorably received by LeConte,[7] Penck[8] and others, and no reason to question its truth has thus far appeared.

The summary objection first enumerated is therefore invalid.

The common second objection of the first two reviewers has also been anticipated by Wallace, whose climate-diagram exhibits but one a-glacial period[9] separating two considerable glacial periods.[10] The improbability that an extensive ice-sheet could be melted during an inter-glacial epoch had been previously shown, quantitatively, by the writer.[11] Since, however, these investigations have been alike neglected in the recent reviews, a more thorough examination must be undertaken.

While the maximum accumulation of polar ice must ever fall below that of middle latitudes,[12] and while independent glaciers may be locally developed in any latitude, provided other conditions be favorable, it may be assumed without discussion that the Quaternary ice of the northern hemisphere originated well within the arctic circle; and here, accordingly, may the manner and rate of development of an ice-sheet be considered.

Again: while glacial periods must, as is generally admitted, be periods of low temperature, and must also, as long ago demonstrated by Tyndall,[13] be characterized by abundant precipitation, the recognition of these conditions as causative and not induced is unwarranted in the present state of knowledge; while, as best elucidated by Wallace,[14] geographical vicissitudes unquestionably affect the formation of polar ice, the evidence of such changes of magnitude commensurate with the phenomena of Quaternary glaciers wholly fails; and while as urged by Croll,[15] continental configuration may serve to indirectly intensify glacial conditions, its agency must ever be secondary and

[7] "Elements of Geology," 2d ed., 1882, 578.

[8] "Die Vergletscherung der Deutschen Alpen," 1882, 452.

[9] Since the term "inter-glacial" was used in a definite and restricted sense by Croll, it seems desirable that some other expression should be employed to denote any considerable period, or number of periods, during which glaciers did not prevail; and for this purpose "a-glacial" is suitable.

[10] "Island Life," 122. [11] Geol. Mag., vi, 1879, 418.

[12] Cf. "Maximum Synchronous Glaciation," Proc. Am. Assoc. Adv. Sci., xxix, 1880, 447, *et seq.*

[13] Cf. "Forms of Water," 1877, 154. [14] "Island Life," Ch. ix.

[15] "Climate and Time," Am. ed., 1875, Ch. v; Geol. Mag., vi, 1879, 480; Geikie's Text Book, 1882, 27; and elsewhere.

subordinate. In the eccentricity theory, *per se*, then, all save the immediate effects of increased eccentricity, under conditions similar to those actually known to obtain, must be eliminated; and on these further premises may the mode and rate of ice-accumulation be sought.

In the north-frigid zone the existing ice-fields are to all appearances permanent; whence annual addition to them from congealed precipitation and loss from melting, flow of ice and water, and the liberation of bergs are practically equal. The annual precipitation can only be approximately estimated. If on the last edition of Loomis' rain chart[16] the precipitation on land areas be the means of the values represented by the several tints employed, the average for the year at N. lat. 68° is 13·3 inches. Toward the pole it must be materially less: it is, indeed, sometimes so little in northern Greenland that Bessels thought the glaciers there must be but remnants of those formed during past ages.[17] The mean (and the measure of melting) for the whole year certainly cannot exceed 10 inches.

Neglecting trivial amounts from diverse sources, the heat reaching the frigid zone is derived (1) from vapor-laden winds, and (2) from direct solar accession. Now that received from the first of these sources is indeterminate; but that from the second is alone sufficient to liquefy 399 inches (33·26 feet)[18] annually. Actually not more (and probably far less) than $\frac{1}{40}$ of this melting can take place, and it is hence manifest that in computing the effects of eccentricity, the actual and not the theoretical values of annual addition and loss must be employed.[19] The source of the discrepance need not here be considered in detail.

For convenience, and since no appreciable error will be introduced thereby, the foregoing values for precipitation and melting, and their equality, may be assumed normal—i. e., such as would obtain were the solstices equidistant from the apsides.

Different investigators have shown that the immediate result of increased eccentricity (in conjunction with precession) must

[16] This Journal, xxv, January, 1883.

[17] Cited by Woeikof, "Winds of the Globe," Smithsonian Contrib. Knowl., 268, 1875 (=vol. xx, 1876), 680.

[18] Cf. "Maximum Synchronous Glaciation," op. cit., 473.

If, as appears from a popular notice (Harper's Magazine, lxvii, 1883, 91, cf. this Journal, xxv, 1883, 195), Langley's recent observations prove (1) that the value of solar accession hitherto accepted is too low, and (2) that the temperature of stellar space approaches absolute zero, the argument here presented will gain in force, and greater potency must be ascribed to the agencies contemplated in the eccentricity theory.

[19] In the discussion already alluded to ("On the Superficial Deposits of the Mississippi Valley," Geol. Mag., vi, 1879, 418) the theoretical rate of melting was used as a basis for an estimate of the maximum removal of ice during an interglacial period; and the result is accordingly far too large.

be the inauguration of five glaciation-factors, of which two are direct and three indirect. These are, (1) diminution of mean temperature,[20] (2) increase of evaporation and precipitation,[21] (3) acceleration of radiation,[22] (4) promotion of reflection,[23] and (5) increase of protection from solar accession by clouds and fogs.[24] The first factor has been roughly evaluated only as regards its effect on temperature, whence its agency in ice-accumulation remains indefinite; and the quantitative efficiency of the last four has never been sought, and cannot now be numerically expressed without more thorough investigation than is here warranted. It must therefore suffice to assume for all a probable value;[25] and it will assuredly do no violence to the most conservative opinions (especially in view of the considerable influence shown to be exerted by the first factor) to assume that the united agency of the five factors is such as to counterbalance the lessened solar distance in summer, and render the combined factors effective in the sum of the excess of winter-season above normal. Manifestly, since but two (and probably not the most important) of the glaciation-factors are directly effective, while the others acquire efficiency only through the accumulation of ice, the diminution of annual melting will not be uniform but cumulative throughout the Platonic winter—the period during which winter-season occurs in aphelion; as the ice increases, so will its disposition to increase become intensified; but the assumed may be regarded as the mean rate. During each Platonic winter, then, a mean accumulation of ice directly proportional to the increase in length of winter-season will annually take place.

With the eccentricity of 210,000 years ago (·0575, as computed by Croll)[26] the maximum excess of winter-season over the normal during such Platonic winter was about 13·5 days, or a mean for each such period of 6·75 days; throughout which period, accordingly, the annual accumulation of ice was $\frac{6{\cdot}75}{365}\times 10$, or ·185 inch, and within which the total accumulation was equal to ·185×13,000, or 2405 inches (200 feet).

Toward the equator actual and possible annual precipitation and liquefaction progressively increase, the first four glaciation-

[20] The writer, "A Contribution to Croll's Theory," this Journal, xxii, 1881, 437.
[21] Hill, "Evaporation and Eccentricity," Geol. Mag., viii, 1881, 481; this Journal, xxiii, 1882, 61.
[22] Croll, "Climate and Time," 1875, 58; Geikie's Text Book," 1882, 25.
[23] Croll, op. cit. 1, 60; op. cit. 2, 26.
[24] Croll, op. cit. 1, 60; op. cit. 2, 26.
[25] Let it be clearly understood that such an assumption is not made as an attempt to demonstrate the validity of the eccentricity theory by any process of defective reasoning; the only demonstration worthy of the name, now admissible, would be an approximate evaluation of the glaciation-factors, severally and jointly, at the various latitude of the zone over which they are efficient. Such an investigation presents no serious difficulty aside from the time and labor required in its prosecution.
[26] "Climate and Time," 320.

factors progressively lose in relative efficiency, the fifth increases, the share of heat derived from warmer latitudes diminishes, and the periodicity of solar accession becomes more equable, whereby loss through radiation is accelerated. From this complex of diverse and antagonistic elements only the most vague estimates of the relative rates of addition and loss in higher and lower latitudes could be directly deduced without exhaustive analysis and computation; but it is certain that the annual addition to the ice-sheet could never exceed the precipitation, while it is obvious that the annual loss must fail of the addition; whence the foregoing value, if doubled or tripled, and certainly if quadrupled, would be ample for the whole glaciated area of the northern hemisphere.

But however slow the rate of ice-accumulation, the rate of ice-dissolution, when the positions of the hemispheres were reversed, would be far slower; for the effective factors of accelerated radiation and promoted reflection, which are but secondary in the initial development of an ice-sheet, would then reach maximum efficiency. The enormous dissipation of heat by icy surfaces is seldom adequately appreciated: after a light snow-fall equal to but a fraction of an inch of ice, in the upper Mississippi valley, the temperature falls from freezing-point to zero, and the snow is not even softened by a day's uninterrupted sunshine demonstrably sufficient to melt an inch and three-quarters of ice; the névé-fields of the Savoyan Alps receive enough solar energy in a year to melt 54 feet of ice, yet the actual superficial liquefaction must be trivial; an earlier paragraph indicates that less than a fortieth of the theoretical melting actually occurs in the frigid zones; the solar accession in the frigid zone in summer is considerably greater than at the equator, as Meech has calculated,[27] yet the liquefaction annually effected there would be effected in a week were the available energy utilized in such work; it appears susceptible of mathematic proof that if the water of the earth were converted into a mantle of ice uniformly enveloping its surface, not a crystal of it would ever be melted. Not even an approximate estimate of the rate of ice-dissolution can be made without analyzing and evaluating the wonderfully effective agencies of radiation and reflection; but it is certain again that only a fraction of the ice accumulated during a Platonic winter could be removed during a Platonic summer; and whether the fraction be large or small, it may safely be affirmed that little more than a tithe of the Quaternary ice-sheet could have been removed during a single inter-glacial period.

Let it be observed that if the preceding estimates of present

[27] "Relative Intensity of the Light and Heat of the Sun," Smiths. Contrib. Knowl., 85, 1855 (=vol. ix, 1856), 18, pl. I.

annual precipitation, or of the combined values of the glaciation-factors, be excessive, the computed rate of ice-accumulation is too rapid; while if they be defective, the importance and efficiency of eccentricity as an element in glacial climate has been underestimated.

The foregoing results sustain the opinion of Wallace and others as already stated, and show that (presumably) the weightiest objection of the recent reviewers is invalid.

The third objection of the first reviewer is based on a subordinate side-question springing indirectly from the Crollian theory, which may or may not in any way affect the fundamental principles of the theory; for the question as to what physical effect a given mass of ice will exert on the earth's center of gravity and on the position of the ocean is wholly independent of the question as to the reason of Quaternary ice-accumulation; and the validity of the eccentricity theory, *per se*, is accordingly in no way affected by the verity of the phenomena adduced.

Again, the tripartite sequence of Quaternary deposits described in the text-book (glacial—aqueous—glacial) does not appear to be so thoroughly understood and so clearly drawn, and the consensus of opinion concerning it so uniform, as to allay the suspicion that the aqueous beds may be analogous to those everywhere deposited during and immediately after the withdrawal of the second ice-sheet.

This objection, therefore, is also invalid.

In his first objection the second reviewer overlooks the fact (upon which the writer has already had occasion to insist)[28] that the hypothetical deflection of the Gulf Stream, in the manner contemplated by Croll, is an *effect* of glaciation, and, if a cause at all, only a secondary one. Hence if the glaciation-factors alone are capable of inaugurating a glacial period, the assistance of this element is not essential; and if they are not alone capable of producing such an effect, the whole theory fails.

This objection, too is accordingly incompetent.

With no desire to underestimate the actual difficulties of the eccentricity theory, or to detract one iota from the laudable caution displayed in such general criticism as that of the first reviewer, a word may be added with reference to the depreciatory tone of the class of critics represented by the third reviewer. Intricate and far-reaching as the theory is, encroaching as it does upon different branches of science, involving as it must elements seldom coördinated by individual specialists, it requires for its thorough comprehension a range of preliminary study

[28] "Croll's 'Climate and Time'" (review), Popular Science Monthly, xvi, 1880, 819.

which few geologists can afford to bestow upon it; and as with special investigations generally, so in this case, the men who have not made such study are prone to ignore or disparage both the investigation and its results. It assuredly speaks strongly for the respectability, and equally makes for the probability of the theory, that nearly every geologist whose writings show that he thoroughly comprehends it is disposed to regard it as something more than a vague hypothesis, and that those who understand its principles best are most ready to teach it as a tentative but probable geologic and cosmogonic doctrine. Never more, and seldom as much, may be said of the narrower speculations of empirical geology.

Salt Lake City, Utah, April 15th, 1883.

ART. XV.—*On the Commingling of ancient Faunal and modern Floral Types in the Laramie Group;* by CHARLES A. WHITE.

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THE fact that fossil plants of Tertiary types have been found in strata of the Laramie Group associated with Dinosaurian remains has been long known and undisputed, even by those who strongly contend for the Cretaceous age of that group. Concerning the true Laramie age of some of the collections of plants, however, that have been published as coming from strata which are now known to belong to that great group, many of those who are interested in the matter have felt some doubt, and others have persistently discredited it. In the present consideration of this question I shall make special reference to those strata of northeastern Montana and adjacent parts of Dakota, to which Dr. Hayden gave the name of Fort Union Group, and to the large collection of plants which he made from its strata. Descriptions of these plants were published by Dr. J. S. Newberry, in 1868, in volume ix of the Annals of the New York Lyceum of Natural History, pp. 1–76; and his illustrations of the same were published by Dr. Hayden, in 1878, in connection with the U. S. Geological Survey of the Territories. Dr. Newberry then referred the species of the plants in question to the Miocene epoch; and while most of them were then new, he identified a part of them with known European Miocene species. So distinctly characteristic of the Tertiary period were the types of those plants generally admitted to be that the assignment which Dr. Newberry made of them has, I believe, never been seriously called in question. Dr. Hayden at the same time and from the same localities, col-

lected many invertebrate fossils which were afterward described by Mr. Meek and himself; and some years afterward Professor Cope obtained from the same region numerous vertebrate remains, among which were those of Dinosaurs. Mr. Clarence King, accepting the conclusions of Dr. Newberry as to the Miocene Tertiary age of the plants here referred to, and those of Professor Cope as to the Cretaceous age of the Dinosaurs, expressed the opinion that the strata from which the former were obtained were distinct from those of the latter and of much later geological age. The following paragraph is quoted from his volume I, of the U. S. Geological Survey of the 40th Parallel, p. 353: "The relations of conformity or nonconformity between the plant-bearing beds of Fort Union, and the Dinosaurian beds are not given, and there is reason to believe that the plant beds represent a horizon of the great White River Miocene series which underlies the Pliocene over so large a part of the Great plains. * * * * I apprehend that the plant-horizon at Fort Union will be found to be nothing but the northward extension of the White River Miocene." My reason for referring especially to this statement of Mr. King is that it occupies a place in the leading volume of one of the larger series of U. S. Government geological publications, and it is therefore (although he distinctly states that he has never visited the region in question), liable to mislead those who have not personally studied the geology of that region, if it should pass unchallenged.

During the summer of 1882 I gave especial study to the geology of the region about Fort Union, extending up the Yellowstone Valley, and including all the localities from which Dr. Hayden obtained the fossil plants here referred to. The result of that study has been to ascertain that only one formation, namely, the characteristic Fort Union Group, which is nothing more or less than a part of the great Laramie Group, occupies that whole region. That is, with the exception of one or two small exposures of the Fox Hills Cretaceous Group, upon which the Laramie strata rest conformably, no other than Laramie strata are to be found there.*

At numerous points along the valleys of the Yellowstone and Missouri, including many of Dr. Hayden's localities, I collected fossil plants, shells and vertebrate remains. The molluscan shells are those which belong to characteristic species of the Laramie Group, examples of most of which I have collected from its strata in Wyoming and Colorado, on both sides of the Rocky Mountains. The following is a list of

* In the last May number of this Journal I gave an account of a small deposit which I found on the top of a butte about 100 miles south of Fort Union, which may be of later age than the Laramie Group; but in that deposit only a few fossil fishes were found, and it is several hundred feet above the fossil plant horizon.

them as identified by myself, comparing them with the type specimens or other authentic examples:

Unio priscus Meek & Hayden.
Unio senectus White.
Corbula mactriformis M. & H.
Thaumastus linnæformis M. & H.
Viviparus trochiformis M. & H.
Viviparus Reynoldsianus M. & H.
Viviparus Leidyi M. & H.
Viviparus retusus M. & H.
Viviparus Lleai M. & H.

Professor E. D. Cope, who has examined the vertebrate remains which I collected with the foregoing mollusca, recognizes the former as belonging mostly to the same species which he had previously obtained from that region; and he gives me the following general determinations of them:

Dinosaurians, two species.
Camposaurus, one species.
Chelonians, three species. (*Trionyx*, *Emys* and *Clastomenus*.)
Ganoids, one species. (*Clastes*.)

Mr. Lester F. Ward, who has charge of the fossil plants of the U. S. Geological Survey, and who is making a special study of those of the Laramie Group, has given me a list of 35 species which he has identified to his entire satisfaction with species heretofore published. The larger part of these are species which Dr. Newberry published, as before mentioned. From that list I select the fourteen species named below for the purpose of illustrating the subject of this article:

PARTIAL LIST OF FOSSIL PLANTS OBTAINED FROM THE LARAMIE STRATA OF THE YELLOWSTONE VALLEY.*

Yellowstone valley.	*European.*	*Other Laramie localities.*
Equisetum arcticum Heer.	Miocene, Spitzbergen.	
Sequoia Langsdorffii Heer.	" "	Black Buttes, Sout'rn Wy.
Phragmites Œningensis A. Br.	" Switzerland.	" " "
Gymnogramma Haydenii Lesqx.	Golden Colorado.	
Sparganium stygium Heer.	" Switzerl'd & Green'ld.	
Corylus Americana Walt. (living).		
Corylus rostrata Ait. (living).		
Platanus Guillelmæ Göpp.	" Greenland.	[den, Colorado.
Magnolia tenuinervis Lesq'x.		Bl'k Buttes, Wy. and Gol-
Elæodendron Helveticum Heer.	" Europe.	
Rhamnus Gaudini Heer.	" "	
Juglans rugosa Lesq'x.		Black Buttes, S. Wyom'g.
Juglans bilinica Ung.	" "	
Trapa microphylla Lesq'x.		Point of Rocks, S. Wy.

Three important facts are shown by this partial list of the fossil plants from the Laramie strata of the Yellowstone valley.

* The other species of plants found associated with these are, as before stated, of recognized Miocene types, but they are omitted from this list because, so far as the plants are concerned, reference is made especially to *specific* classification.

First, two of the species are among the most common living North American shrubs. Second, eight of them are identified with European Miocene species. Third, six of them are identified with those which have been found in unquestionable Laramie strata in Colorado and Southern Wyoming. Now, I assert without hesitation, that all the plants from which the foregoing list has been made were found in such association with the molluscan and vertebrate remains that have been mentioned in this article as to show plainly that they all lived contemporaneously. That is, if all these identifications are correct, as they are believed to be, two species of living plants, eight species of acknowledged Miocene plants, numerous fresh-water mollusks of living types, and two existing genera of Chelonians, all co-existed with Dinosaurs.

It is not to be denied, if there were any inducement to do so, that the Dinosaurian remains are found toward the base of the Laramie Group, in the Yellowstone valley, and that the principal horizon of the fossil plants is somewhat above that of the Dinosaurs there. But all the plants of the foregoing list have been found associated together, and some of them also extend below the upper known limit there of the Dinosaurs. Some of the other vertebrate remains herein named are found associated with the Dinosaurs, and the range of the former extends up into the principal horizon of the plants. There is no trace or suggestion of a separation of any of the strata which contain all these remains into separate formations, and there is no unconformity whatever among those strata. Furthermore, I have found at the Southern Wyoming localities named in the foregoing list of plants, Dinosaurian remains in layers above those which contain the plants. At least two of the latter, be it remembered, are identical with European Miocene species. The specific identity of the molluscan and plant remains leaves no possible room for doubt as to the equivalency of the Fort Union strata of the Yellowstone valley with the typical Laramie strata of Southern Wyoming that have been referred to.

Art. XVI.—*Notes on some Fossil Plants from Northern China;* by J. S. Newberry.

Mr. Arnold Hague recently placed in my hands a small collection of fossil plants brought by him from China. They proved to be interesting, and with his permission I present briefly the results of my examination of them.

The circumstances under which they were found, so far as known, are given in the subjoined notes of Mr. Hague which accompanied them:

"This collection of plants came from the coal basin of the Pin-hsu-hoo, in the southern peninsula of Mantchuria, on the east side of the Gulf of Liantung, and about one hundred miles northeast of the open port of Niu-chwang. I am told that at times there are over 1200 Chinamen engaged in the district in mining and transporting coal. This coal field has long been known to foreigners through the Chinese as a possible source of workable coal. As long ago as 1863 Professor Pumpelly suggested that the Liantung coals should be examined by American or European experts before opening the mines at Chaitang, which he had personally visited and reported upon favorably.

So far as I know no geologist has visited the district except Baron v. Richthofen, who regarded the formation as of Paleozoic age, although I believe he found no fossils.

From my own observations while traveling through the provinces of Chihte and Shansi, and from various sources of information, I believe by far the greater part of the coal basins of North China are of Paleozoic age, although the well-known districts west and northwest of Pekin have been shown to be of Mesozoic age.

The estimates of the great area and value of the coal and iron deposits of North China, which have been made by Professor Pumpelly and Baron Richthofen, are, I think, by no means unwarranted.

There can be no question but that the coal and iron of China will prove to be of immense value in the material development of the country so soon as she decides to adopt railways and foreign engineering methods."

On unpacking the collection I discovered that the plants were of Carboniferous age and that most of them belonged to species common in the rocks of Europe and North America.

Of the ten species which can be distinguished, one is a *Pecopteris* too imperfect for determination (probably *P. unita* Brgt.), and two others, a *Lonchopteris* and an *Archæopteris*, present slight differences from their closely-allied representatives in Europe and America. The other seven are undistinguishable from what may be considered as the most characteristic plants of our Coal-measures.

The complete list of species is as follows:

Annularia longifolia Brgt.
Sphenophyllum oblongifolium Germar.
Calamites Suckowii Brgt.
Cordaites borassifolius Ung.
Lepidodendron obovatum Sternb.
Sigillaria Brardii Brgt.
Pecopteris Cyathea Brgt.
Pecopteris, unita? Brgt.
Archæopteris, n. sp.?
Lonchopteris, n. sp.?

The *Archæopteris* indicates that the coal with which these plants were associated belongs near the base of the Coal-measures, as this genus does not rise above that horizon.

The species of *Lonchopteris* and *Archæopteris* are best represented in the collection, and the former is very well shown. In general aspect it is not unlike the figures given by Brongniart of his *L. rugosa*, (Veg. Foss., p. 368, tab. 131, figs. 1, 2, 3), but the pinnules are smaller and the reticulation much more open. In the latter respect it is more like *L. Baurii* Andr., *L. Eschweilerianus* Andr., and *L. conjugata* Gœpp., sp. (*Neuropteris conjugata* Goepp.), but it has narrower, more pointed and curved pinnules than either.

Should a larger number of specimens show that these are constant characters it will be necessary to regard this as a new species which may be fittingly named after Mr. Hague, *Lonchopteris Hagueana*. The *Archæopteris* mentioned is a very graceful and well-marked species of the genus, having obovate or spatulate pinnules, of which the upper extremities are often crenulate or fimbriate. It is less robust than the type forms of *A. Hibernica* Forbes, sp., and the pinnules are more symmetrical. It is about the size of *A. Jacksoni* Dwn., but has less crowded, more elongate, and more regularly ovate or spatulate pinnules. The nerves are fine, forked only near the base, simple and sub-parallel above. Possibly this may be, like several described species of *Archæopteris*, only a variety of *A. Hibernica*, but the characters mentioned seem to separate it from any known form, and I would suggest for it the name *Archæopteris spatulata*.

The first fossil plants brought from China were collected by Professor Pumpelly, and were described by the writer in the Smithsonian Contributions of 1866. These included two new ferns, *Sphenopteris orientalis* N., and *Hymenophyllites tenellus* N., *Pecopteris Whitbyensis*, found in the Lias and Upper Trias of Europe, *Podozamites lanceolatus*, a Liassic plant of the old world, and *P. Emmonsi*, which occurs in North Carolina, all of which seem to represent the Upper Trias or Lower Lias.* Subsequently (in 1868), M. Ad. Brongniart exam-

* My paper on the fossil plants collected by Professor Pumpelly has been reviewed by Heer (Juraflora Ostsiberiens, p. 17), and Schenk (Richthofen's China, vol. iv, p 264), with some suggestions in regard to their generic and specific relations which would hardly have been made had these distinguished paleontologists had access to the specimens on which I based my conclusions. Having examined these fossils I take occasion to offer here a few additional notes upon them.

Sphenopteris orientalis certainly belongs to the same genus with the ferns now called *Thyrsopteris Murrayana* and *Th. Maakiana* Heer, the latter being specifically hardly different from it. *Hymenophyllites tenellus* N., has finely-dissected pinnules, and is certainly distinct. My *Podozamites lanceolatus* is that plant and not a *Phœnicopsis* as suggested by Heer. This is shown by its nervation, and by the fact that the pinnules are pinnately set on a rachis and are not fasciculate as in

ined a collection of plants obtained from southern Shansi and gave a list of them in the *Bull. de la Soc. Geol. de France*, 3d Series, vol. ii, p. 408. They included some of the species collected by Pumpelly, and were considered by M. Brongniart to represent the Upper Trias and Lower Jura. More recently Baron v. Richthofen obtained fossil plants from various parts of China, and these have been described by A. Schenk in vol. iv of Richthofen's China. They represent two distinct horizons, one Carboniferous and the other Mesozoic. The former were found in the districts of Shansi and Hunan. Here were obtained *Pecopteris cyathea*, *P. unita*, *Annularia longifolia* Brgt., *A. maxima* Schenk, *Sphenophyllum emarginatum* Brgt., *S. Schlotheimii* Brgt., *Calamites gigas* Brgt., etc. Elsewhere in the provinces of Shansi and Tshili, Richthofen obtained a group of Mesozoic plants, among which M. Schenk recognized *Pecopteris Whitbyensis*, *Podozamites lanceolatus*, and other species which led him to refer the strata containing them to the Brown Jura.

It is known to most geologists that the extensive coal basins of India, from which fossil plants have been described by Oldham and Morris and Dr. Feistmantel, are all of Mesozoic age. The same is true of the coals of Tonking, Cochin China, from which a considerable number of fossil plants have been obtained by the French expeditions and described by M. R. Zeiler in the *Annales des Mines*, October, 1882.

It would seem proven, therefore, that the coal basins of China (in which the coal is very largely converted to anthracite by local metamorphism), belong to two great geological systems, one, as indicated by the plants collected by Baron Richthofen and Mr. Hague, the equivalent of the Coal-measures—and probably the entire range of the Coal-measures of Europe and America—the latter not yet capable of so definite classification, but probably referable to both the Rhætic and Lias.

The fossil plants brought by Mr. Hague from China, representing as they do, not only the characteristic genera of the Coal-measures in Europe and America, but identical or closely allied *species*, cannot fail to interest both geologists and botanists; the first, by the confirmation they afford of the classification adopted for the stratified rocks, based on the fossils they contain; the latter, from the evidence they furnish of

Phœnicopsis. *Taxites spatulatus* N. is the leaf of a conifer, and not of a cycad, as inferred by Heer. It has but a single nerve, the median, which is strong and traverses its entire length, and has a wedge-shaped base terminating in a short, *twisted* petiole. The publication of Heer's important paper on the Jura-flora of Eastern Siberia has given significance to certain specimens in Pumpelly's collection and has enabled me to add to the list of species *Baiera angustiloba* Heer (very near to *B. Munsteriana*), *Phœrucopsis longifolia* Heer, and *Czekanowskia rigida* Heer.

the practical identity of the acrogenous flora of the Coal period over so large a portion of the earth's surface, and the remarkable persistence which specific characters exhibit in the wide range of migration and the incalculable lapse of time through which the dispersion was effected.

Since none of the higher plants were in existence upon the earth's surface during the Carboniferous age, wherever a terrestrial flora prevailed, it could only be composed of acrogens and gymnosperms, but how it happens that within these limits there was so little diversity is incomprehensible. During the Coal-measure epoch the same genera, and to a large degree the same species, seem to have lived in North America, Europe, Brazil and China.

No one who has any acquaintance with fossil plants would fail to recognize at once most of the species in the collection brought by Mr. Hague; but if shown seven out of the ten he could not say whether they came from America, Europe or Asia. And yet, in the interval between the deposition of the Coal-measures and the Triassic rocks the whole flora of the globe was revolutionized. Before the Bunter was laid down *Lepidodendron*, *Sigillaria*, *Annularia*, *Sphenophyllum*, *Cordaites*, and indeed all the characteristic forms of the coal flora had disappeared. The Cycads in great variety, true Equiseta, and peculiar genera of conifers and ferns, gave new aspects to nature, and this again over the whole world. From the desert of Atacama, from Sonora, New Mexico, North Carolina, Europe, India and China, we obtain the remains of the unmistakable Mesozoic flora with species which are common to all these widely separated localities.

Hence, we are safe in fixing by fossil plants the geological horizon of the Mesozoic coal basins of China; but the identity of species in the Mesozoic flora, though surprising, is not quite so marked as in that of the Carboniferous age.

In the Middle Cretaceous came another great revolution, and the angiosperms succeeded the gymnosperms so rapidly that the earth was covered with oaks, magnolias, willows, etc., before the Tertiary began. Gradually we are gathering the details of this wonderful history, and ultimately we shall be able to report the *facts* with a good degree of fulness; but the causes which inspired the revolutions that have taken place in plant life, and the processes by which these great changes have been effected seem to be as inscrutable as ever.

ART. XVII.—*Review of DeCandolle's Origin of Cultivated Plants; with Annotations upon certain American Species;* by ASA GRAY and J. HAMMOND TRUMBULL.

IN concluding, after some unexpected delay, our observations upon DeCandolle's *L'Origine des Plantes Cultivées*, it may not be improper to state that, as we learn, the work was planned upon a somewhat larger scale, which was reduced to suit the requirements of the publisher. This accounts for the omission of certain articles which would otherwise have found place.

PART III.

Lycopersicum esculentum, Tomato.—We have only to note an oversight in respect to the Mexican cultivation of the *Mala Peruviana*, as it was named by some botanists of the 16th century. DeCandolle refers to Humboldt's statement that the cultivation of this esculent was ancient in Mexico, but adds that there is no mention of it in the earliest work on the plants of that country, viz: Hernandez, *Historia.* But Hernandez (ed. 1651, p. 295, ff.) actually has a chapter "De *Tomatl*, seu *planta acinosa* vel *Solano*, and describes several sorts under their Mexican names.*

Persea gratissima, Alligator Pear of the English, *l'Avocat* of the French; a singular corruption of a native name, as De Candolle remarks, which had no more to do with an alligator than with a lawyer. Our author does not carry back the native Mexican name quite to its original, which was *Ahuacahuitl*, corrupted by the Spaniards into *Aguacate*, *Avogade*, etc. Champlain, who saw it in Mexico in 1599 or 1600, calls the

*I find only *one* writer in the 16th century who gave the Tomato a name indicating a Peruvian origin—namely, Anguillara, whose treatise "De Simplicibus" was first printed, in Italian, at Venice, 1561. On his authority the name "*Poma Peruviana*" is introduced, in the synonymy, by C. Bauhin, in his annotations on Matthioli, 1598 (p. 761), and the *Hortus Eystettensis* (attributed to Besler), published in 1613, is referred to by the same writer (*Pinax*, 1671, p. 167) as authority for the names, *inter alia*, "Poma amoris fructu rubro & *Mala Peruviana*." The *Hortus Eystettensis* is the only authority cited for "*Mala Peruviana*" in the work to which M. DeCandolle refers—the *Historia Stirpium*, attributed to J. Bauhin, but published long after his death (in 1551), with large additions by his son-in-law Cherler, and by Chabræus and Graffenreid. Guillandinus, of Padua, in a treatise "De Papyro," 1572, named the "*Tumatle Americanorum*," as a species of *Pomum Amoris* or *Solanum pomiferum*;" and, earlier, Matthioli had described it (*Comment. in Dioscor.*, ed. 1559, p. 537) as a "kind of Mala insana" [Solanum melongena], which was "beginning to be imported" into Italy, and which was "popularly called *Pomi d'oro*, that is, *Mala aurea*."

Anguillara may have confounded the Tomato with another of the American Solanaceæ, introduced at about the same period—the Thorn Apple (Datura Stramonium) which Guillandinus (1572) named "*Mala Peruviana*," and the French called "*Pomme de Perou*." J. H. T.

fruit *Accoiates* and *Acoyates;* Voy. to the W. Indies [Hakluyt Soc., 1859], p. 28.

Oviedo described "the wild pear-tree of the main land," in 1526. It grew "in the province of Castillo del Oro (Panama), in the sierras of Capira and the country of the cacique of Juanaga," etc. In the revision of his first work, in 1535, he adds, that he had, some years before, seen these trees cultivated by the Indians in Nicaragua (*Hist. gen. y nat.*, lib. ix, c. 22). It was still a tree of "Terra firma"—not yet introduced into the Islands. Clusius saw it in a garden in Valencia—"said to be brought from America"—thirty-five years earlier than the date (1601) mentioned by DeCandolle. He described the "Persea" in the *first* edition of his *Historia rariorum Stirpium*, 1576 (lib. i, c. 2), published five years after his journey in Spain.

Passiflora.—This genus is wholly omitted by DeCandolle; unaccountably so, considering how much Granadillas have been cultivated and prized in tropical countries. A note on the subject may not be out of place, as a species was cultivated by our own Indians.

As to early history and aboriginal nomenclature, Monardes (*De Simplicibus Medic.*, c. 66) says that the Granadilla was spontaneous in Peru, and that the fruit was highly esteemed by the Indians and by the Spaniards. Cieça de Leon (*Chron. del Peru*, c. 28), about 1550, saw it growing in the valley of the Rio Lile, near Cali (now in Colombia) and in the country about Pasto. The flower and fruit are mentioned by J. de Acosta (*Nat. and moral Hist. of the Indias*, b. iii, c. 28)—"the fruit sweet, and too sweet, in the opinion of some." Lery (1557–8) does not appear to have found it in Brazil, but it was common there before the middle of the 17th century. Piso and Marcgrav (*Hist. Nat. Brasil.*, 1648, pp. 106, 70) reckon nine species or more, of which four were cultivated—two, especially, for their fruits. The Tupi (Brazilian) name was *Mburucuîâ* (Montoya, 1639) which Piso and Marcgrav, *l. c.*, wrote *Murucuia;* the species which was generally cultivated for its fruit was *Mburucuîa-guacu* (i. e. great *Murucuya*). Father R. Breton (*Diction. Caraibe*, 1665) gives *Merécoya* as the Carib name of the fruit; but this seems to have been adopted from the Tupi—for in that language *Mburucuîa* denotes the "fruit of a vine."

It is remarkable that the Tupi (and Carib) name went with the plant and its fruit to the country of the Algonkins, before the coming of Europeans. One species (*Passiflora incarnata*) was cultivated by the Indians of Virginia. "They plant also *Maracocks*, a wild fruit, like a lemon, which also increase infinitely" (Capt. John Smith, *Gen. Hist.*, p. 29); and, again (p. 25), Smith mentions the "fruit which the inhabitants call *Mara-*

cocks—pleasant, wholesome fruit"—among "things which are *naturally* in Virginia." Strachey (*Travaile into Virginia*, 72, 119) describes the "fruit called by the natives a *maracock*, which the Indians plant," etc.

Although no longer planted, the fruit of the spontaneous plant is still eaten in the southern Atlantic States; and its popular name, *May-pop*, is probably the last stage of the Tupi original.

Now our *Passiflora incarnata* is so like *P. edulis* (well known in cultivation), the home of which is in Brazil,* that botanists have been unable clearly to distinguish the two, except by the fact that ours, dying down to the ground at approach of winter, remains herbaceous. It occurs in a rather narrow geographical range; and Dr. Masters, in his elaborate study of the order (*Trans. Linn. Soc.*, xxvii, 641; see also *Flora Brasiliensis*), says that "being so far separate from the remainder of its allies of the same subgenus, [it] may be considered as an outlier." Altogether we may infer that the fruit and the name were originally derived from the same South American source.

Musa, Banana.—The author concludes, as did Robert Brown, that Banana and Plantain are varieties of one species; also that this species is of the Old World; that in all probability it was not known in the West Indies when discovered by Columbus; but that in respect to the western side of North America, there is some evidence which is not easily ruled out, especially the statement of Garcilasso that the Peruvians had the banana before the conquest, and of Stevenson ("Trav. in S. Amer.") who is said to have seen in the ancient Peruvian tombs beds made of Banana leaves. This is discredited because the author found beans in the same tombs, "et que la fêve est certainement de l'ancien monde." But if Stevenson wrote *beans*, without doubt he meant the seeds of Phaseolus, not of Faba. It would rather seem that the Banana, like the Sweet Potato and Cocoa-nut had early been transported over the Pacific.

Phaseolus vulgaris, Kidney Bean.—Three weeks after his first landing in the new world Columbus saw, near Nuevitas in Cuba, fields planted with "*faxones* and *fabas* very different from those of Spain," and two days afterwards, following the north coast of Cuba, he again found "land well cultivated with these *fexoes* and *habas* much unlike ours." "*Faxones*" or "*fexoes*" were—as Navarrete notes, *Colec.* i, 200, 203,—"the same as *frejoles* or *judias*," Spanish names for kidney beans, which the Portuguese call *Feijaos*. Oviedo (1525–35) speaks of

* Not Mexico, although indeed said to have been brought from "New Spain" to the garden of the Farnese palace, in Rome, as early as 1619. It is described, under the name of *Maracot*—with excellent plates showing the plant, the flower, and the fruit—by Tobias Aldinus, in *Rarior. Plant. Horti Farnesiani* (Romæ, 1625), pp. 49–59.

the "*fésoles*, as the Spaniards call them, of which there are many kinds in the [West] Indias." These *fésoles*, he says (lib. vii, c. 18) "are called by Pliny *fagivoles:* in Aragon we call them *judias*, and the seeds of those of Spain and of this country are properly the same." The natives of Hispaniola raise these *fésoles*, but they are much more abundant on the main land, especially in New Spain and Nicaragua. "I have, in the province of Nagrando in Nicaragua, seen them gather a hundred hanegas (bushels, nearly), of these *fésoles:* and they also, in that country and other parts of that coast, have many other kinds of *fésoles*, besides the common sort: some have yellow seeds, others spotted," etc. In another place (lib. xi, c. 1), Oviedo, mentioning plants that had been brought from Spain to Hispaniola and other parts of America, "in the beginning," names "*Fésoles*, called in Aragon *Judias*, and in my country [Castile] *Arvejas luengas:*" but "of these, there is no need of bringing more seed, for in this island and on the main many bushels are harvested every year, and in the province of Nicaragua they are indigenous (*naturales de la misma tierra*), and a great number of bushels are produced yearly of these and of other *fésoles* of other sorts and different colors," etc.

From this time (1535) onward, nearly every writer who mentioned plants cultivated by the Indians named, together or in close connection, maize, *beans* and pumpkins. Reference to several of these writers has heen made in our notes on *Cucurbitæ*. Cabeça de Vaca found beans cultivated by the Indians of Florida in 1528, and again, near the western limit of his wanderings (in New Mexico or Sonora) in 1535. De Soto, at his landing in 1539, found "fields of maize, *beans* and pumpkins," near Tampa Bay; and at Coligoa (west of the Mississippi) "beans and pumpkins were in great plenty; both were larger and better than those of Spain;" and so, at other places, on his travel to the west and north.

Jacques Cartier, the discoverer of the St. Lawrence, on his first voyage, 1534, found that the Indians near the mouth of that river on the Bay of Gaspé had abundance of maize, and had "beans (*febues*) which they name *Sahu*," or (as spelled in the vocabulary printed with his *Discours du Voyage*) *Sahe*.* The *Bref Recit* of his second voyage, 1535–6, mentions the use of corn and beans by the Indians of the St. Lawrence—"bled & *febues* & *poix*, desquels ilz ont assez" (f. 24).

Father Sagard in his History of Canada and in the account of his journey to the country of the Hurons, 1625, mentions the

*The language spoken by these Indians was a dialect of the Huron-Iroquois group, and we trace the name *sahe* (as Cartier caught it) in the Mohawk *osahe-ta* "fésoles" of Bruyas (17th century) and the Onondaga *ousahèta* and *hôsahèta* "poix, féve" (Shea's *Onondaga Dictionary*).

cultivation and use of "fezolles" by the Indians. The Hurons used in their succotash (*neintahouy*) "a third or a quarter part of their *fezoles*, called *ogaressa*" (Grand Voyage, 83, 138).

Lescarbot, 1608, says that the Indians of Maine, like those of Virginia and Florida, plant their corn in hills, "and between the kernels of corn, they plant beans marked (*féves riolées*) with various colors, which are very delicate; these, because they are not so high as the corn, grow very well among it" (*Hist. Nouv. France*, ed. 1612, p. 835; see also, p. 744).

The relation of the voyage of Captains Amidas and Barlow to Virginia, 1584, mentions *pease*, melons, etc., at Roanoke Island, but does not name beans; but Harriot, who accompanied them on this voyage, includes both "*Wickonzour*, called by us Pease," and "*Okindjer*, called by us Beans," among the productions of that country. Capt. John Smith, who was in Virginia in 1607, and Strachey, who was there in 1610, describe (in nearly the same words) the Indian manner of planting corn and beans: "they plant also pease they call *assentamens*, which are the same they call in Italy *fagioli:* their beans are the same the Turks call *garvances*, but these they much esteem for dainties" (Smith's *Gen. Hist.*, 28; Strachey, *Trav. in Virginia*, 117). Evidently, these names are confounded. *Garvance* was the French name of the Chick Pea (*Cicer arietinum*), the Spanish *garbanzo;* and it is not probable that the "Turks" gave this name to any kind of beans; while *fagiuoli* was the Italian equivalent of Latin *phaseoli.* Strachey's Virginian vocabulary gives *assentamens* (and *otassentamens*) for "pease," and *peccatoas*, *peketawes*, for "beans."

It must be remembered that at the beginning of the 17th century, kidney beans—as well as vetchlings (*Lathyrus*)—were popularly regarded as a kind of pease, or "peason." Turner, in his *Names of Herbes*, 1548, says that "*Phasiolus*, otherwyse called *Dolichos*, may be called in English *long peasen* or *faselles;* . . . . in French *phaseoles*": and "*Smilax hortensis*, ... in French, as some wryte *Phaseole* . . . . may be called in English *Kydney beane*," etc., (*Engl. Dial. Soc.*, ed. 1881, p. 62, 74). Lyte's Dodoens, 1578, follows Turner for the English names of Phaseolus, "Kidney beane and Sperage; of some they are called Faselles, or Long Peason," etc. (p. 474): his "common Peason" and "middle Peason" are *Ervilia* (*Ervum Ervilia* L.?) and *Pisum arvense* L.; while *P. sativum* is distinguished as "Great Peason, Garden Peason, and Branche Peason, because, as I thinke, they must be holpen or stayed up with branches" (*id.* 476).

So, on the continent, the Spanish names for "*fésoles*" was "*arvejas luengas*" (Oviedo) i. e., 'long vetches,' and the garden pea (*P. sativum*) was "un corto genero de Arvejas" (Calepin's

Diction., ed. 1616), a short kind of vetches. The confusion of names is frequent in writers of the 16th and first half of the 17th centuries, in French, as well as English and Spanish. "Magno sane labore."—as Tragus found—"*Phasioli, Orobi*, et *Pisa*, necnon *Cicer arietina*, maxime agrestia, secernuntur," *Stirp. Hist.*, 1552, p. 613.

Champlain uses *poix* and *fèves*, interchangeably, as names of the American *Phaseoli.* In Breton's *Diction. Fr.-Caraibe* (1666) the Caril name, *caláoüana*, stands for "pois de Brésil" and "febue de Brésil"; "pois rouges, dit Anglois, *mîbipi*"; and "Pois, *mancónti*"—this being, probably, the (introduced) *P. sativum.*

Sagard (1624–5) says that the Hurons called the coarser part of their pounded maize—after the meal had been sifted from it—"*Acointa*, c'est à dire Pois (car ils lui donnent le mesme nom qu'a *nos pois*);" and in his *Dictionnaire Huronne*, he has "Pois, *Acointa*," "Fezolles, *Ogaressa*;" whence we infer that *French* pease [*P. sativum*] were already cultivated by or known to the Hurons. The Abnakis of western Maine, in the 17th century, called pease, *awennootsi-minar*, i. e. "French (or foreign) seeds." Tanner, 1830, gives as the Chippeway name of the "Wild pea vine" [Phaseolus diversifolius?] *Anishemin*, i. e. 'Indian (or, native) seeds.' In nearly all North American languages, the names for kidney-beans (*Phaseoli*) are of *earlier formation* than those for garden pease. The latter are usually formed *on* the former: e. g. Chahta, *tobi*, bean; *tobi hullo*, [wild] pea; *tobi hikint ŭni*, garden pea (Byington): Dakota, *o^{n}mnicha*, bean; *o^{n}mnicha hmiya^{n}ya^n* [i. e. 'round bean'], pea.

Without multiplying citations—we may assume that the "pease" and "poix" which early voyagers found cultivated by the American Indian were species of *Phaseolus*—not *Pisum.*

Five and twenty years before the settlement of Virginia, the Indians of Carolina and Florida had "fine citroüilles and very good *beans*" (Lescarbot, *Nouv. France*, 778). Lawson, 1700–1708, gives a more particular description of the southern beans cultivated by the Indians: "The *Kidney-Beans*," he says, "were here *before the English came*, being very plentiful in Indian cornfields." "The *Bushel* bean," a spontaneous growth, very flat, white, and mottled with a purple figure, was trained on poles [*P. multiflorus?*]: "Indian *Rounceval*, or Miraculous Pulse, so called from their long pods and great increase; they are very good, and so are the *Bonavies*, *Calavancies* [= *Garvances?*], *Nanticokes*, and abundance of other pulse, too tedious too mention, *which we found the Indians possessed of when first we settled in America*" (*Voyage to Carolina*, pp. 76, 77).

In the northern States, we find little difficulty in establishing

the identity of *Phaseolus vulgaris* with the beans cultivated by the Indians at the first coming of Europeans. These were, from the first, distinguished, as "*Indian* beans," from the garden beans (*Vicia Faba*) introduced by the English. In 1609, Hudson, exploring the river which bears his name, saw at an Indian village—in the vicinity of Schodac and Castleton, Rensellaer county, N. Y.—"a great quantity of maize or Indian corn, and *beans* of the last year's growth" (Hudson's *Journal*, in De Laet, 1625, b. iii. ch. 10, and Juet's, in Purchas: *N. Y. Hist. Soc. Coll.*., 2 Ser., i. 300, 325).

1631–42. The Indians of New Netherland "make use of *French beans* of different colours, which they plant among their maize. . . . The maize stalks serve, instead of the poles which we use in our Fatherland, for the beans to grow upon" (De Vries, *Voyages*, transl. in 2 *N. Y. Hist. Soc.*, iii. 107).

1653. Van der Donck, in his "Description of the New Netherlands," distinguishes the beans cultivated by the Indians before the coming of the Dutch, and the *Turkish beans* which had been introduced: "Of Beans there are several kinds; but the large Windsor bean [*Vicia Faba*] . . . and the horse bean will not fill out their pods. . . . The Turkish beans which our people have introduced there grow wonderfully. . . . *Before the arrival of the Netherlanders* [1614] *the Indians raised beans of various kinds and colours*, but generally too coarse to be eaten green or to be pickled, *except the blue sort*, which are abundant," etc. He then describes the Indian mode of planting beans with maize, *ut supra* (*N. Y. Hist. Soc. Coll.*, 2 Ser., i. 188–9). This is the only reference we have found to the *introduction* of any species of *Phaseolus*, into North America. Van der Donck's book was written more than forty years after Hudson's coming, and the author first arrived in New Netherland in 1642. His statement as to the introduction by the Dutch of the best kind of "Turkish beans" for "snaps," salad, or pickling, is not to be accepted without reserve; but the fact that Turkish beans "grow wonderfully, fill out remarkably well, and are much cultivated," while the imported Windsor beans [*Vicia Faba*] and horse-beans proved failures is to be noted.

Wood, who was in Massachusetts from 1629 to 1633, says that the Indians, "in winter-time have all manner of fowles, *Indian beanes*, and clams" (*N. E. Prospect*, pt. 2, ch. 6). Roger Williams, 1643, gives the Indian name of these beans, in the Narraganset dialect: *Manusqussed-ash* (plural); Cotton's Massachusetts vocabulary (1727–8) has (sing.) "*Mónasquisset*, an Indian bean;" President Stiles, about 1760, heard the name in the Pequot dialect as *Mushquissedes* (MS. Vocab.); Zeisberger, 1776 and 1803, wrote it in the Delaware, with dialectic modification, *Malachxit;* and we can trace it in the modern Shyenne *Mónisk*

(Hayden's Vocab., 1862) and *Monchka*. In the Chippeway, the kidney-bean has received—probably from some local variety—a different name: *Miskodissimin*, i. e. 'red-dyed seed (or fruit);" and this name, modified as *M'skochī-tha*, was used by the Shawanoes of Ohio.

To return to New England, Josselyn, who was in this country, 1638–9, and again, 1663–71, in his catalogue of "plants proper to the country, names "*Indian Beans*, falsely called *French* beans:" "the herbalists call them kidney-beans, from their shape and effects. . . . They are variegated much [in size and colour]; besides your *Bonivis* and *Calavances*, and *the kidney-bean that is proper to Roanoke:* but these are brought into the country: the others are *natural to the climate*" (*N. E. Rarities*, p. 56; *Voyages*, p. 73–4). Here is reference to at least *two* species of American beans, one "proper to New England," the other from Roanoke—perhaps *P. multiflorus*.

Besides the names already mentioned—*Mônasquisset*, with its variants—there is another, in northern Algonkin languages, for kidney-beans, which must have originally belonged to some *high-twining* variety. Eliot used it, in the plural, for "beans" in 2 Samuel, xvii. 28, *tuppŭhquam-ash*—which literally signifies 'twiners;' and Rasles (1691–1700) gave, in the Kennebec-Abnaki of Maine, for "faséole, *a'teba'kwé*—from the same root. A modern Abnaki vocabulary shows that this name is still in use—as "*ad-ba-kwa*."

As to the American origin of *P. lunatus*, the Lima bean and its varieties, there seems now to be no question. "It is evidently," says M. DeCandolle (p. 276), "a Brazilian species, dispersed by cultivation, and perhaps long ago naturalized, here and there, in tropical America."

But as to the origin of *P. vulgaris* he is not free from doubt. He finds "(1) that this species was not cultivated in ancient times in the East Indies, the southwest of Asia, or Egypt; (2) that we are not absolutely certain that it was known in Europe before the discovery of America; (3) that at that epoch the number of varieties suddenly increased in the gardens of Europe, and all authors began to speak of it; (4) that the majority of species of this genus are found in South America; and (5) that seeds which apparently belong to this species have been found in Peruvian tombs [at Ancon] of a date somewhat uncertain, mixed with many other species, all of which are American" (p. 275).

The proof that *P. vulgaris* (and *P. nanus*), in varieties almost innumerable, were cultivated by the natives of America, before the coming of Europeans, seems to be conclusive. The resolution of M. de Candolle's doubts as to the American origin of the species must depend, chiefly, on the identification of the species known as *Phaseoli* (*Phaseli*, *Fagiuoli*, *Fésoles*, etc.), in

Europe, *before* the discovery of America. This identification may not be impossible, but the space at our disposal will not permit us to attempt it in this article, or even to re-examine the authorities on which M. de Candolle admits the probability "that the *Dolichos* of Theophrastus was our pole bean (*haricot à rames*), and the *Fasiolos* our cultivated bush bean (*haricot nain*"), p. 271. At present, we have only to offer one or two notes—

1. The distinction indicated by Galen (*de Alimentis*, lib. i, cc. 25, 28), between the *Phasiolos* (φασίολος), of Dioscorides and *Phaselus* (φάσηλος)—presumably the "vilis faselus" of Virgil—if well founded, seems to have been lost sight of in the middle ages. In Italy, the Greek and Latin names *Phasiolos*, *Faseolus*, *Faselus*, *Fasillus*, etc., passed into the modern *Fagiuoli*.

1.

Piero de' Crescenzi, of Bologna, whose treatise on agriculture was written near the beginning of the 14th century, in Latin, and translated into Italian about 1350, mentions, among field plants, *Faseoli* (*Fagiuoli*), as well known; "some of them are red, some white. .... They are planted, conveniently, among panick, millet and chick pease; they are also planted in gardens, among cabbages and onions."* It is not certain that the red and the white were of the same species, or genus, or that either was a species of *Phaseolus* L. In the first half of the 16th century the white Phaseoli were the more common and less esteemed. The young and tender pods were eaten, with the included seeds, in salads, or boiled with other vegetables.

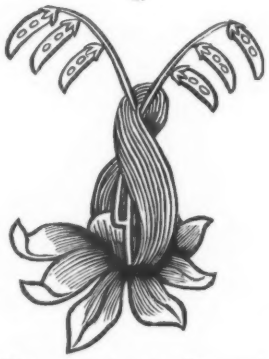
Two other early figures show that the *Faseoli* were not so "well known" to the herbalists of the 15th and beginning of the 16th century as to Crescenzi in the 14th. One (fig. 2) is from a Venice edition of the *Hortus Sanitatis*, 1511; the other (fig. 3) from the *Tacuini Sanitatis* of "Elluchasem Elimithar," Strasburg, 1531, p. 49. They are equally unlike the modern *Phaseolus*, the earlier figure in Crescenzi, and each other. Fig.

* *De Agricultura*, lib. iii, c, 10 (Italian, Ed. Venice, 1504). The Latin text was first printed at Strasburg, in 1471, and with *figures*, 1486; the Italian version was printed at Florence, 1478. The figure (fig. 1) of *Faseolus* in the earliest (Latin) edition we have seen, without date, but probably of Louvain, about 1480, has little resemblance to the *Phaseolus* of modern botany.

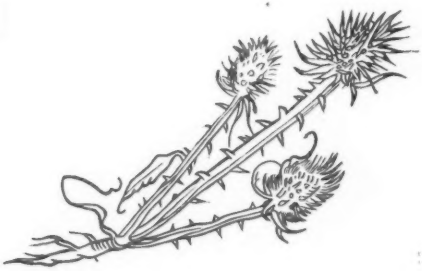
M. de Candolle remarks (p. 272) that "authors of the 15th century say nothing of *Faseolus*, or any analogous name," and that "this is the case with P. Crescenzi,"—referring to a French translation of Crescenzi, printed in 1539, which we have not seen.

3 may have originally been intended for a Teasel (*Dipsacus sylvestris*) the *Virga pastoris* of the herbalists. Calepin's Dictionary (ed. 1616) says, s. v. *Faseolus*, that the name *Fasilli* is now given by the common people, to "a species of *Cicercula*."

2.



3.



This "vulgaris Phaseolus" of Matthioli* and other Italian botanists of the period, is figured and described in the later editions of his commentary on Dioscorides,† as having an erect stem, ternate leaves, white flowers, kidney-shaped seeds, white, "*except the umbilicus, which grows black.*" It may be worth noting that "a black spot in the place of the cotyledon" was a characteristic of the *Faseoli* described by Albertus Magnus (13th cent.), which appears to M. de Candolle "to be the dwarf Haricot (*P. nanus*) of our epoch." This black spot is more strongly suggestive of *Dolichos* than of any known variety of *P. vulgaris*: e. g., *Dolichos unguiculatus* L. (French, *D. Mongette, Banette, Haricot cornille*)—not mentioned by M. de Candolle, but much cultivated in Italy, and of which there are a great number of varieties—which has seeds "marked by a prominent black spot, about the umbilicus."‡

2. M. de Candolle (p. 272), with a reference to Delile and to Piddington's *Index*, remarks that though "no Hebrew name corresponding to the *Dolichos* or *Phaseolus* of the botanists" is known, yet "a name less ancient, because it is *Arabic*, namely *Loubia*, is found in Egypt for the *Dolichos Lubia;* and in

* "Vulgares Phaseoli, quibus passim in cibis vescimur, dum satis in campis virent, *non repant*," etc., Matth. *Apologia adv. Amathum*, 1559, p. 33; "Vulgaris usus Phaseolus." *Ibid.* 31.

† *Ed.* C. Bauhin, 1598, p. 341. In the earlier edition (*Venice*, 1559, p. 264), *Phaseolus* is figured as a low, bushy and spreading, but not twining plant.

‡ Several other species of *Dolichos* (e. g., *D. sesquipedalis* L., Ital. *Fagiuolo Sparagio*, Engl. Asparagus bean) are similarly marked. Vilmorin-Andrieux et C^ie^, *Les Plantes Potagères*, 1883, p. 280. Other names for this species are: "*Germ.* Ostindische Riesel-Spargel Bohne, Nagelische Fasel; *Ital.* Fagiuolo dell' occhio; *Span.* Garrubia, Moncheta, *Judia* de Careta."

Hindustani, under the form *Loba*, for *Phaseolus vulgaris*." This name seems to be clearly referable to the Greek. It has not been traced earlier than to Jahia ebn Serapion—an Arabian physician of the 9th or 10th century—whose work "de Simplicibus" compiled chiefly from Dioscorides and Galen, was translated into Latin in the 15th century.* In a chapter (lxxxi) on "*Lubia*, i. e., Faseoli," he quotes from Dioscorides, the description of *Smilax hortensis* (*κηπαία σμίλαξ*) "whose seeds some call *Lobia*"; and it is evident that the name *Lubia* (as it was transliterated from the Arabic text by the translator) was *transferred to the Arabic* from the Greek of Dioscorides. It is probable, to say the least, that it has been rightly appropriated to *Dolichos Lubia* Forskal (De Candolle, 278), rather than to any species of *Phaseolus*.

The length to which our annotations have extended forbids all notice of the third part of this book. This, however, is very brief. It contains a tabulation of the plants of cultivation, and of the results of the preceding discussion of them; also an article on the regions in which the principal species have originated or have been brought into cultivation, in which it is stated that of the 247 species under investigation the Old World has furnished 199, and America 45, leaving three which are doubtful in this regard; and the extreme poverty of the southern hemisphere beyond the tropics is a striking feature. An article on the number and nature of species cultivated at different periods is noteworthy. So, also, is the enumeration of the cultivated plants which are unknown in a wild state; from which it is gathered that 27 species have never been found wild by any botanist, 27 more are doubtful in this respect, while 193 are of recognizable origin.

Of the "Reflexions diverses," at the close, we note only the final one, that "In the history of cultivated plants, I have found no indication of communications between the inhabitants of the Old and New World anterior to the discovery of America by Columbus. The Scandinavians, who had carried their expeditions to the northern United States, and the Basques of the Middle Ages, who had extended their whaling voyages perhaps to America, would appear not to have transported a single cultivated species. The Gulf-stream has equally been without effect. Between America and Asia two transportations may have been effected, one by man (the *Batatas*), the other either by man or by the sea (Cocoa-nut)."

Perhaps the Banana should be ranked with the Sweet Potato in this regard. And we may merely conjecture that the Purslain came to our eastern coast with the Scandinavians or the Basques.

* Milan, 1473, and Venice, 1479; but better known to botanists of the 16th century in the Strasburg edition of 1531, edited by Otho Brunfels.

ART. XVIII.—*On the Supposed Human Foot-prints recently found in Nevada;* by O. C. MARSH.*

DURING the past summer, various accounts have been published of the discovery of human foot-prints in sandstone near Carson, Nevada. The locality is in the yard of the State prison, and the tracks were uncovered in quarrying stone for building purposes. Many different kinds of tracks were found, some of which were made by an animal allied to the elephant; some resembled those of the horse and the deer; others were apparently made by a wolf. There were also tracks made by large birds.

1.

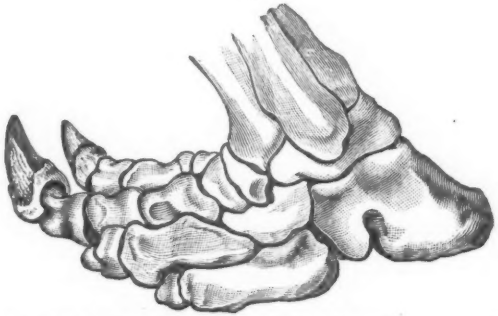


Figure 1.—Left hind foot of *Mylodon robustus* (after Owen). One-sixth natural size.

2.

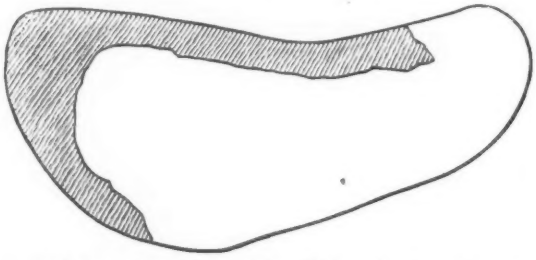


Figure 2.—Left foot-print at Carson (after Harkness). One-sixth natural size.

The foot-prints occur in series, and are all nearly in the same horizon. Some of the smaller tracks are sharp and distinct, but most of the impressions are indefinite in outline, owing apparently to the fact that the exact surface on which they were made is not usually exposed.

* Abstract of a paper read before the National Academy of Sciences, at New York, November 17th, 1882.

The supposed human foot-prints are in six series, each with alternate right and left tracks. The stride is from two and one-half to over three feet in extent. The individual foot-prints are from eighteen to twenty inches in length, and about eight inches wide. The distance between the line of right hand and left hand tracks, or the straddle, is eighteen to nineteen inches.

The form and general appearance of the supposed human tracks is shown in figure 2, which is a reduced copy of one of the impressions represented by Dr. W. H. Harkness, in his paper before the California Academy of Sciences, August 7th, 1882. The shaded portion was restored by him from other foot-prints of the series. A copy of this impression was given, also, by Professor Joseph LeConte, in his paper before the same society, August 27th, 1882.

The size of these foot-prints, and especially the width between the right and left series are strong evidence that they were not made by men, as has been so generally supposed.

A more probable explanation is that the impressions are the tracks of a large Sloth, either *Mylodon* or *Morotherium*, remains of which have been found in essentially the same horizon. In support of this view it may be said that the foot-prints are almost exactly what these animals would make, if the hind feet covered the impressions of those in front. In size, in stride, and in width between the right and left series of impressions, the foot-prints agree closely with what we should expect *Mylodon* or *Morotherium* to make. In figure 1, the bones of the left hind foot of a species of *Mylodon* are represented, the figure being reduced to the same scale as the accompanying cut, figure 2, of one of the supposed human foot-prints.

The geological horizon of these interesting foot-prints is near the junction of the Pliocene and Quaternary. The evidence, at present, appears to point to the Equus beds of the upper Pliocene as the nearest equivalent.

Since the above communication was read, the writer has had an opportunity of examining photographs and casts of the Carson foot-prints, and is confirmed in his opinion that the supposed human tracks were made by large Edentates. The important fact has recently been determined that some of these tracks show impressions of the fore feet. The latter are somewhat outside of the large foot-prints, as would naturally be the case, if the animal changed its course.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Contributions from the Chemical Laboratory of Harvard College during the Academical Year* 1882–83.—Professor C. Loring Jackson and Mr. A. E. Menke in continuation of their work on turmeric have prepared the mono and di acetcurcumin, and shown that the rich purple substance formed by the action of phosphoric oxychloride on curcumin has a composition which differs at most but very slightly from curcumin and is probably an anhydride formed by removing one molecule of water from two of curcumin. By oxidizing turmerol with a solution of potassic permanganate they have obtained two acids, $C_{11}H_{14}O_2$ and a second a melting at 221° C. which has probably the composition $C_{10}H_{10}O_4$. The first of these acids throws some light on the position of the hydroxyl group in the molecule of turmerol.

Professor Jackson and his assistant have also studied the action of phosphorous chloride on aniline and have shown that the product first formed is probably $C_6H_5NHPCl_2$, although they have not succeeded in isolating this body. Upon heating the crude product a material was obtained which yielded on treatment with alcohol $(C_6H_5NH)_2POH$. This last substance has been studied carefully and its composition indicates that the intermediate product was $(C_6H_5NH)_2PCl$.

An investigation of the constitution of camphor undertaken by the same chemists failed so far as its main object was concerned, but it led them to a method of preparing borneol from camphor much simpler, easier and giving a larger yield than either of those now in use. This method consists in dissolving camphor in common alcohol and adding sodium. One crystallization from alcohol is sufficient to give pure borneol, and the yield is essentially that demanded by theory.

Professor Jackson and Mr. G. T. Hartshorn have studied the sulphur derivatives of parabrombenzyl, including the sulphide, sulphone, mercaptan, mercaptide and disulphide of this radical; as well as the potassium, calcium, barium and lead salts of the corresponding sulpho-acid and also the chloride of its radical. Descriptions and analyses of these several products will be found in a paper recently published by these authors in the Proceedings of the American Academy of Arts and Sciences.

In order to obtain data for determining the constitution of pyromucic acid, Professor H. B. Hill has begun the study of its substitution products. Thus far he has examined only a monobrompyromucic acid melting at 184°–185° C. While under certain conditions oxidizing agents, such as nitric acid or bromine water, convert this substance into fumaric and carbonic acids, it also appears that under slightly different conditions the action of bromine leads to the formation of dibromsuccinic acid and isodibromsuccinic acid, besides a neutral body, dibromfurfuran-

tetrabromide, which when treated with the alcoholic potash gives tetrabromfurfuran. This result is especially interesting because these two last bodies are derivatives of Limpricht's so-called tetraphenol.

Professor Hill with Mr. E. K. STEVENS has also studied the action of alkalies upon mucobromic acid. Professor Hill, in connection with Mr. O. R. JACKSON, had previously found that a large excess of an alkaline hydrate decomposed mucobromic acid into formic and dibromacrylic acids, the last passing also into brompropiolic acid. Quite a different reaction ensues if care is taken to keep the solution barely alkaline. There is then formed the salt of a dibasic acid, which is so unstable that it cannot be isolated. The analyses of its salts show that this acid is formed from mucobromic acid by the replacement of Br by OH, and it is therefore isomeric with brommaleic acid. Both the free acid and its salts give a deep purple red coloration with ferric chloride and reduce argentic oxide when warmed in contact with this reagent. Bromine water at 100° C. gives oxalic acid and bromal hydrate. Alkalies in the cold produce no further change, but on boiling oxalic and carbonic acids are formed. Professor Hill and Mr. Stevens do not feel warranted at present in giving a definite structure formula to the new acid. They have tried without success to obtain an analogous product by using sodic ethylate. Potassic phenylate, however, reacts readily on mucobromic acid, and replaces bromine by the phenoxy group. But this phenoxy compound shows more analogy to mucobromic acid itself than to the hydroxyl derivative just described. With alkalies it is converted into formic acid and phenoxybromacrylic acid. This last acid shows great stability and the circumstance that the Br atoms can neither be replaced or split off as HBr, indicates its constitution.

Professor Hill and Mr. MORRIS LOEB have made from mucochloric acid the corresponding phenoxychloracrylic acid.

Dr. C. F. MABERY obtained γ dichlordibrompropionic acid by the addition of chlorine to β dibromacrylic acid. By the action of a solution of baric hydrate, the elements of hydrobromic acid were eliminated from the γ acid and a dichlordibromacrylic acid was formed. Two other dichlordibromacrylic acids were prepared by the action of baric hydrate on the α and β dichlordibrompropionic acids respectively. By the addition of chlorine to brompropiolic acid a bromtetrachlorpropionic acid was formed. Chlorine was also taken up by chlorbromacrylic acid; and from the resulting trichlorbrompropionic acid, baric hydrate abstracted the elements of hydrobromic acid, forming trichloracrylic acid. With chloride of bromine an addition-product of brompropiolic acid was obtained, a solution of brompropiolic acid in chloroform absorbing chloride of bromine to nearly the extent required by theory. In order to determine the structure of the resulting chlordibromacrylic acid the elements of hydrobromic acid were removed from the chlortribrompropionic acid by the action

of baric hydrate; but whether the chlordibromacrylic acid thus obtained is identical or isomeric with the other remains to be determined. The study of the acids thus obtained by Dr. Mabery is still in progress.

Dr. L. P. KINNICUTT and Mr. G. M. PALMER have studied the β phenyltribrompropionic acid. This acid was obtained by the addition of dry bromine to the β bromcinnamic acid (melting point 120° C.) The substance is soluble in alcohol, ether and chloroform. By recrystallization from chloroform it can be easily purified, and when so obtained melts at 151° C.) It is decomposed by boiling water, giving the α bromcinnamic acid (melting point 132° C.); together with a new acid, very soluble in water and melting at 184° C., which is beyond doubt a phenyldibromlactic acid, and an oil which boils with slight decomposition at 253°–255° C. This oil is a dibromstyrol which when exposed to bromine vapor forms an addition-product. This addition-product, itself a very thick oil, is probably a tetrabromstyrol, and we expect that the study of the products formed when this last body is treated with boiling water, or with alkalies, will show the constitution of the dibromstyrol, and by inference that of the monobromcinnamic acid whose melting point is 120° C. and which is commonly called the β acid, although the position of the bromine in the molecule has never been satisfactorily determined.

In the course of his work Dr. Kinnicutt has devised a method of preparing carbonic oxide gas (CO) which is a valuable addition to our methods of preparing this well known aeriform substance, and forms a striking lecture-room experiment. Dr. E. Noack has lately shown that carbonic oxide can be obtained by passing carbonic dioxide over heated zinc-dust. His results suggested the idea that the same gas might be more conveniently made by heating carbonates directly with zinc dust, and it was found that when pulverized magnesite, mixed with twice its weight of zinc dust, was heated in a copper retort, a rapid evolution of carbonic oxide gas took place, which after the first few minutes came over very nearly pure, containing less than one per cent of carbonic dioxide. The magnesite used was that from Eubœa in Greece, which is imported here for making artificial stone, but any form of anhydrous magnesic carbonate would serve as well.

Mr. JOHN N. NEF, of the Junior class, has made, under the direction of Dr. Kinnicutt, some experiments on the determination of nitrous acid by potassic permanganate. The determination of nitrous acid by adding a very dilute solution of the acid to a weak solution of potassic permanganate often proves troublesome and inaccurate, owing most probably to the slow oxidation of the last traces of nitrous acids. It was found, however, that very accurate results could be obtained by the following modification of the process. To a solution of a nitrite in water, two or three drops of dilute sulphuric acid are added, then immediately an excess of potassic permanganate, the volume of the standard solution used being accurately measured. Next ten to fifteen cubic

centimeters of pure H_2SO_4 previously diluted with its own volume of water and cooled, are carefully poured in under constant stirring, and the excess of potassic permanganate is then determined with a standard solution of oxalic acid. Five determinations of a sample of potassic nitrite showed a variation of only $\frac{7}{1000}$ of one per cent, and the same number of determinations of sodic nitrite a variation of only $\frac{5}{1000}$ of one per cent.

In a careful series of experiments, Mr. R. PENROSE, of the Junior class, attempted to apply the same method, under Dr. Kinnicutt's direction, to the estimation of the sulphites, but he did not obtain satisfactory results.

The Director of the Laboratory, Professor J. P. COOKE, has continued his work on the Revision of the Atomic Weights. He has elaborated methods for determining with great precision the relations H : O : Ag; H : S : Ag; H : Cl : Ag.; H : I : Ag; H : Br : Ag, fixing the relations between the members of each triad in a single series of determinations. Starting with the relation Ag : Br = 108 : 80 in regard to which there is a universal agreement he is seeking to fix the relations of each value at once to two others, whose mutual relations are already known, as in his previous work on the relation,

$$Ag : Br : Sb = 108{\cdot}00 : 80{\cdot}00 : 120{\cdot}00.$$

In this way he hopes to be able to control so effectually the experimental errors that the questions in regard to the hypothesis of Prout or the constancy of the atomic weights may be finally settled. This investigation obviously involves a great amount of labor; and since it must be a long time before the final results can be reached this announcement is made to prevent an unnecessary duplication of the work. In connection with this investigation the writer has developed a simple method of correcting the weight of a body for the buoyancy of the atmosphere, which was described in the previous number of this Journal, and similar subordinate results will be published from time to time as the investigation proceeds.

Under the direction of Professor Cooke, Mr. C. P. WORCESTER, of the graduating class, has determined the vapor density of the chloride, the bromide and the iodide of antimony by means of the displacement process of Victor Meyer with the following results, which are in each case the mean of three experiments:

	Density, Air=1.	Theory, SbR_3.
Antimonious chloride,	7·96	7·85
" bromide,	12·57	12·48
" iodide,	17·59	17·33

The first two values were obtained by displacement of air, the last by displacement of nitrogen, and the variations from theory do not appear to be greater than the errors inherent in the process. It therefore appears that in the state of vapor the molecules of all three compounds have the same simple constitution.

Mr. Worcester also determined the heat evolved in the familiar

reaction of zinc on dilute sulphuric acid. By experimenting with zinc dust, which dissolves with great rapidity when in contact with platinum, and previously determining the proportions of zinc and oxide of zinc mixed in the powder, it was found possible to conduct the reaction under such conditions that the heat evolved could be measured in a calorimeter. The values obtained were considerably greater than those calculated from the well-known heat of combination of sulphuric acid and zinc sulphate, and the results were interesting chiefly as controlling the more fundamental thermo-chemical data, and bringing to light discrepancies which must be reconciled before entire reliance can be placed on this new class of chemical constants. Investigations similar to this were undertaken by other students, but without equal success, and the results are reserved until sufficient data have been obtained to warrant positive conclusions.

Mr. S. H. KNIGHT, of the graduating class, re-determined the boiling point of sulphur by the method contrived by the writer, and previously described in this Journal by Mr. Z. R. Bennett. His experiments give as the mean value of many observations at the normal pressure of the air 447°·9, with a probable error of two-tenths of a degree, a value which is several degrees higher than that recently given by Professor Crafts. But although Mr. Knight's results agree very closely with that previously obtained by Mr. Bennett with the same method, it is not improbable that in both cases the boiling point may have been abnormally raised by the super-heating of the vapor, by impurities in the sulphur, or by the effect of the vessel used. The vessel, made of cast iron, was lined with a porcelain-like glaze, and directly heated with a gas flame.

Mr. OLIVER W. HUNTINGTON has determined the crystalline form of chlordibromacrylic acid, $C_3ClBr_2O_2H_3$, obtained as above by Dr. Mabery. The crystals are oblique prisms and have a well-marked triclinic habit. The prismatic axis was taken as the axis z and the axes of the only other zones capable of giving close measurements were taken as the axes of x and y. The elements of the crystal were calculated from the fundamental angles 010 on 100=66° 23′, 100 on 001=116° 17′, 010 on 001=116° 20′, 100 on 101=63° 50′, 100 on $1\bar{1}0$=47° 5′. The calculation gives

$$a:b:c::=0{\cdot}79807:1:0{\cdot}72072$$
$$x \text{ on } y \quad 104°\ 43'; \quad y \text{ on } z \quad 71°\ 6'; \quad x \text{ on } y \quad 71°\ 10'.$$

Besides the planes above enumerated the following were also determined:

$$430;\ 0\bar{5}4;\ \bar{3}04?\ 7\bar{4}0;\ \bar{4}70;\ 4\bar{7}4;\ 047?$$

Mr. Huntington also has had in preparation a collection of chemical tables, and of these a set of logarithms to five places of decimals, arranged on an ingenious plan to facilitate rapid reference, have been published.

2. *Electricity due to evaporation.*—In order to test the point whether the vapor arising from electrified surfaces of fluids is

positive, negative, or neutral, a number of exhaustive experiments have lately been undertaken in the Physical Laboratory at Berlin by Mr. Blake under the supervision of Helmholtz. It was found that the vapor arising from the electrified surface of water was neutral.—*Annalen der Physik und Chemie*, No. 7, 1883. J. T.

3. *Constant of Dielectricity.*—The electromagnetic theory of light is attracting so much attention that experiments upon electrical stress in dielectrics are becoming of great importance. Quincke has measured the constants of dielectricity and the double refraction of insulating fluids. Using Maxwell's equation,

$$\rho = \frac{K_1}{8\pi} \cdot \frac{P^2}{\alpha^2}$$

in which ρ represents the stress along lines of electrical force, P denotes the difference of electric potentials, α the distance between the plane metallic electrodes between which the fluids are placed, K_1 the dielectricity constant, or the number which represents the increase of the capacity of a condenser when the plates are separated from one another by another insulating substance than air. According to Maxwell, K should be equal to the square of the index of refraction for light waves of infinite length. Quincke's results do not confirm this conclusion. A table of the constant for fifteen liquids is given. The double refraction of insulating fluids is also examined and is found to be variable. It is given in terms of a coefficient defined by Quincke. —*Phil. Mag.*, July, 1883, pp. 1–9. J. T.

4. *Conservation of Solar Energy.*—The June number of the Philosophical Magazine contains an article by E. H. Cook on Siemens's new theory, in which the author proves that CO_2 is distributed uniformly throughout our atmosphere, and concludes that the power of gaseous diffusion is such as to equalize the disproportion of gases which Siemens finds necessary for his hypothesis. Siemens in reply admits the uniform distribution of CO_2 in our atmosphere which is subjected to powerful circulating currents, but does not think that gaseous diffusion can work quickly enough to interfere with his photospheric currents. The effect of the slow action of gaseous diffusion he thinks would be inappreciable upon gaseous currents which arise from solar rotation, which amounts to 4500 miles an hour.

The metallic vapors in the photosphere are chiefly due to mechanical intermixtures. The metallic vapors thus introduced into the photospheric current will be burnt and projected outward into space, not as a vapor as supposed by Mr. Cook, but as a metallic dust much denser than the surrounding medium; the larger portion of which will according to Siemens return to the sun by virtue of solar gravitation and be dissociated on reaching the gaseous metallic sea underlying the photosphere. Siemens then aims to show that the light metallic vapors will be held in suspension in this metallic ocean in the same way that oxygen and carbonic anhydride are retained in sea water. In regard to Mr.

Cook's objection that the preponderance of hydrogen and CO in meteorites is inconsistent with the idea that the meteoric gases have been absorbed in space, Siemens remarks that it is sufficient to know that the meteorites contain all the constituents of our atmosphere except hydrogen: and the latter occurs in our atmosphere combined with oxygen; whereas in space and in the solar photosphere, we have evidence of its separate existence, or in combination with carbon. Mr. Cook's criticism upon the solar fan action is speedily dismissed by Siemens with the imputation that it is not understood by Mr. Cook. It is also pointed out that differences of combustion and richness of gaseous fuel may account for the light of colored stars. In conclusion Siemens refers to a recent paper by his brother, Dr. Werner Siemens, in which the latter cannot reconcile the nature and cause of the electric potential supposed to exist between the sun and the earth except by the adoption of Sir William Siemens' hypothesis. —*Phil. Mag.*, July, 1883, pp. 62–66. J. T.

5. *Metal Microphones in Vacuo.*—The action of the microphone is doubtless a complicated one, and experiments which tend to throw light upon the action between two surfaces in contact at the point where an electrical current passes are of much interest. Mr. J. Munro has constructed a microphone from two fine iron gratings or pieces of gauze which hang loosely against each other in a tube which has been exhausted as completely as possible. The contacts between the wire gratings could be modified by bringing a magnet to the exterior of the tube and thus lifting one grating more or less from the other. The glass tube was placed upon a sounding board and the ticking of a watch placed on this sounding board was very clearly heard in a telephone placed in the circuit. It was thought that the microphone in vacuo acted in an unusually perfect manner, and Mr. Munro thinks that this perfect action of the microphone could not have been supposed from certain experiments of Edlund on the polarization of electrodes in vacuum.—*Phil. Mag.*, July, 1883, pp. 23–25. J. T.

6. *Carbonic acid in air helps in condensing watery vapor.*—Professor Scacchi, in view of the production of clouds of vapor at the larger fumarole of the Solfatara of Pozzuoli by the burning of paper or branches of trees in the mouth (as shown by the guides to travelers), attributed the effect in 1849 to the carbonic acid produced by the combustion, comparing the action to that of hydrochloric acid which fumes in ordinary air. The question has recently been experimentally examined by Italo Giglioli of Portici. The conclusions of this author are as follows: (1) Carbon dioxide helps to condense watery vapor; (2) minute bodies suspended in the air are a powerful cause (the principal cause, as Coulier and Aitken have shown) in the condensation of aqueous vapor; (3) the effects at the fumarole must be ascribed to both the carbon dioxide and the minute carbonaceous particles set free during the combustion.—*Nature* of May 24.

7. *A Treatise on Electricity and Magnetism;* by E. MASCART and J. JOUBERT, translated by E. Atkinson. Volume I, general phenomena and theory. 654 pp., 8vo. London, 1883 (Thos. De La Rue & Co.)—Professor Atkinson has rendered an important service to English students in bringing before them this valuable volume by the French physicists MM. Mascart and Joubert. The entire work is to be embraced in two volumes, of which the first, now published, is theoretical, developing the general principles of the science from the mathematical side; the second is to be devoted to the description of the various phenomena, the methods of measurement and so on. The authors speak of the first volume, forming as it does, essentially, a distinct work, as an "Essay on the Mechanical Theory of Electricity," and this will give some idea of its scope. It is a work of high character, and should be carefully studied by every student of electricity.

II. GEOLOGY AND MINERALOGY.

1. *Elevated Coral Reefs of Cuba;* by W. O. CROSBY, (Proc. Boston Soc. Nat. Hist.)—Mr. Crosby describes, in this paper, the elevated coral reefs of Cuba, and draws from them the apparently well sustained conclusion that they indicate a slow subsidence during their formation, and hence, further, that Darwin's theory of the origin of coral islands is the true theory. The *lowest* reef-terrace, of the northern side of the island, has a height of thirty feet, and varies in width from a few rods to a mile; it was once, plainly, the fringing reef of the shore. The *second* reef-terrace rises abruptly from the level of the lower to a height of 200 to 250 feet, and bears evidence of having been of like origin with the lower. The altitude of the *third* reef is about 500 feet; and the *fourth* has a height east of Baracoa, near the Yumuri River, "of probably not less than 800 feet." These old reef-terraces extend "with slight interruptions—around the entire coast of Cuba; and in the western part of the island, where the erosion is less rapid than farther east, they are the predominant formation, and they are well preserved on the summits of the highest hills. Mr. Alexander Agassiz states that the hills about Havanna and Matanzas, which reach a height of over 200 feet, are entirely composed of reef-limestone."

In the precipitous mountain called El Yunque (the Anvil), five miles west of Baracoa, reef limestone, 1,000 feet thick, constitutes the upper half of the mountain, the lower part, on which the reef rests, consisting of eruptive rocks and slates; and originally the upper limit of this modern limestone formation must have been 2,000 feet above the sea-level. Mr. Sawkins gives 2,000 feet as the maximum thickness of the Jamaica elevated coral reefs above the sea.

Evidence that the reefs were not formed during a progressive rising of the land is drawn from the thickness of the reefs. Mr. Crosby observes that the reefs reaching to a height of 500

and 1,000 feet—if not also that to a height of 2,000 feet show, by the remains within them, that they were made chiefly of reef-building corals, and hence, that they were not begun in deep water, as is assumed in the theory of Mr. Agassiz; but that they were made in shallow water during a progressive subsidence. Mr. Crosby concludes as follows:

"We have then apparently no recourse but to accept Darwin's theory, as an adequate explanation of the elevated reefs of the Greater Antilles; and therefore, to admit that the upheaval of this portion of the earth's crust has been interrupted by periods of profound subsidence during which the reefs were formed. The subsidence of 2,000 feet, of which El Yunque is a monument, must have reduced the Greater Antilles to a few lines of small, but high and ragged islands; and, as Mr. Bland has shown, this fully accounts for the absence in these immense tracts, of all large animals, although they were abundant here in Pliocene and earlier times."

The writer adds here the following objections to the theory of the formation of coral atolls in deep waters out of the calcareous secretions of deep-water life; (1) It is very improbable that submarine eruptions ever make the large and well defined craters, like those of subaerial action, which are appealed to in order to explain the lagoon feature of atolls; (2) many coral atolls are twenty miles or more in diameter, which is vastly larger than the largest of craters; (3) The atolls are never circular, and the larger have the irregularities of outline or diversities of form characterizing other large islands of the ocean; (4) In the actual reefs and islands of the Feejee group (see the map of the Islands in the writer's Corals and Coral Islands), all the conditions, from the first stage to that of the almost completed atoll, are well illustrated, one island having only a single peak of rock within the lagoon, not $\frac{1}{100}$th of the whole area, which a little more of subsidence would put beneath the waters and leave the lagoon wholly free. J. D. D.

2. *Glacier Motion.*—In a paper by W. R. Browne on glacier motion, read before the Royal Society in June, 1882 (and published in Nature, June 5), the author rejects all theories except that of Canon Moseley, which attributes it to change of temperature, and likens the motion to the creeping of a lead plate down a sloping surface. At the close of his paper the author publishes the following Greenland notes by Dr. Rae.

"When in Greenland, in the autumn of 1866, I was ice-bound at the head of one of the fiords, and slept a couple of nights at an Eskimo's house. A glacier about half a mile distant was then in full activity, the movement of which might, I believe, have been as visible to the eye as it certainly was audible to the ear.

My own idea is that Arctic glaciers must have a downward motion more or less during the whole year, summer and winter. I believe the alternation of heat and cold—or, I should rather say, of temperature—would of itself cause motion, especially near the upper surface.

We know that ice two or three feet or more thick contracts very considerably in a few hours by a sudden fall of fifteen or twenty degrees of temperature. I have found cracks in Lake Winnipeg three or four feet wide, formed by this cause during a single night, almost stopping our sledge journey. This gap soon freezes up. Then the weather gets milder, the ice expands, and with the new additional formation is too large for the lake, and is forced up into ridges. This process goes on at every 'cold snap,' alternating with milder weather. Now supposing a glacier for ten or more feet of its depth contracts by cold, as lake ice is known to do, it will get a series of cracks probably in its longest axis, say from inland seaward; the first snowdrift will fill up these cracks or some of them, and this filling up will to some extent perform the same office as the freezing of the cracks in the lakes. The longitudinal extent of the glacier will be increased. A snowstorm always brings milder weather, which would expand the glacier, but as this expansion would naturally tend downhill, instead of up, the whole motion would be downwards. But even if the cracks I mention did not take place, the contraction by cold would pull the ice downhill, not up, while the expansion by increase of temperature would tend to *push* the glacier downhill, so that these opposite actions would produce similar effects in moving the glacier, or such part of it as could be acted upon by external temperature, downwards.

I may also add that when a crack, however slight, is formed by contraction, the cold is admitted into the body of the glacier, and increases the contracting power or influence."

It will be remembered that Professor Agassiz's theory of glacier motion makes the dilatation arising from the freezing of the water that descends into the glacier the chief cause of motion. In his Geological Sketches, published in 1866 (p. 280), he states that other causes of motion have acted, but still urges that dilatation of the mass resulting from the freezing of infiltrated water was an important means.

3. *Glacial cold.*—In the Geological Magazine for July, Mr. S. V. Wood reviews briefly the facts with regard to the ice of the ice period, and the views as to glacial cold, and sustains the opinion he had previously presented that the period was "due neither to a change in the earth's axis, nor to any variation in the eccentricity of the earth's orbit, nor to any change in the distribution of land and water, but to a diminution in the heat-emitting power of the sun."

4. *Contributions to the History of Lake Bonneville;* by G. K. Gilbert. Ann. Rep. U. S. Geol. Survey, 1880–81, pp. 169–200, with plates and maps.—This paper is an abstract of Mr. Gilbert's full but yet unpublished Report on Lake Bonneville. Earlier contributions by him on the lake and its outlet are contained in volumes xv (1878) and xix (1880) of this Journal. The present paper relates prominently to climatic changes which the phases in the lake indicate; the inequality in change of level undergone

about the lake-area, which want of horizontality in the terraces evinces, and the orographic movements that occasioned it; and the relations in time to periods of volcanic activity at points over the region. Mr. Gilbert observes that the highest water-line about Lake Bonneville (the flooded Great Salt Lake and other Utah lakes combined) is 1000 feet above the level of Great Salt Lake; and "over every foot of the intervening profile can be traced evidence of the action of waves." This 1000-foot terrace is called the Bonneville shore-line; and another, which is especially conspicuous, at a level of 400 feet, is called the Provo shore-line. The remarkable terraced features of the region, which skirt all the lower slopes of the mountains, are well shown in plates accompanying the memoir.

The orographic movements about the great lake, according to careful measurements of heights, had their axis near the 113th meridian, the altitudes along the eastern and western shores being about 100 feet below those at intermediate points (at Promontory and Grantsville); and at the extreme southern limit, the southwestern shore of Escalante Bay, the rise in the surface westward was 200 feet in about 25 miles.

Mr. Gilbert deduces from the observations that an increase in elevation of the Wasatch Mountains may be still in progress. A cut represents the great faulting of the strata along the west side of this mountain region, separating its upturned rocks from those that remained nearly horizontal farther west, and states that, in an escarpment showing this line of fault, the time of the latest movement is indicated by the character of the erosion; that the eroded slopes are not a result of subaerial action, but are characterized by the peculiar sculpture of waves; that the last uplift of the Wasatch Mountains along the fault took place after the era of Lake Bonneville and the formation of the Bonneville and Provo shores; and also that the waters of the Great Salt Lake have not since been even 50 feet higher than they are at present. He adds that this uplift is therefore so recent "as to leave no reasonable suspicion that its growth has now ceased."

The memoir closes with the following "summary:"

(1) The climatic episode of which Lake Bonneville was the expression consisted of two humid maxima, separated by an interval of extreme aridity. The second maximum was the more pronounced; the first the longer.

(2) The time elapsed since the close of the Bonneville epoch has been briefer than the epoch, and the two together are incomparably briefer than such a geologic period as the Tertiary.

(3) The period of volcanic activity in the Great Basin, which covered a large share of Tertiary time, continued through the Quaternary also, and presumably has not yet ended.

(4) Such earth movements as are concerned in the molding of continents had not ceased in Western Utah at the close of the Bonneville Epoch, and presumably have not yet ceased.

(5) The Wasatch Range, the greatest mountain mass of Utah, has recently increased in height, and presumably is still growing.

5. *China;* by FERDINAND FREIHERRN VON RICHTHOFEN. Vol. IV. Berlin, 1883.—The fourth volume of Baron von Richthofen's great work on China has just appeared, and is published in the same luxurious style with its predecessors. It forms the paleontological part of the work and consists of descriptions and discussions of the fossils obtained from different parts of China, contributed by various authors: 1, Cambrian trilobites, by Wilhelm Dames; 2, Cambrian brachiopods, by Emanuel Kayser; 3, Middle and Upper Silurian fossils, by the same author; 4, Upper Silurian corals, by Lindström; 5, Devonian fossils of southwestern China, by E. Kayser; 6, Devonian and Carboniferous fossils of Tshau-tiĕn, by E. Kayser; 7, Carboniferous Foraminifera of China and Japan, by Conrad Schwager; 8, Upper Carboniferous fauna of Lo-ping, by E. Kayser; 9, Carboniferous plants, 10, Jurassic plants, 11, Tertiary plants of Southern China, all by A. Schenk.

These memoirs are illustrated by 54 well-executed plates.

In looking through this volume no one will fail to be impressed with the remarkable resemblance and to a large degree identity of the fossils brought by Richthofen from China with those which are most characteristic of the formations holding the same relative positions in Europe and America. Very few new genera are described and a large majority of the species are identical with those previously made known by Hall, Meek, Goldfuss, Römer, Sowerby, Dalman, Angelin, Davidson, Brongniart, Göppert, Heer and others. For example: in twelve species of *Productus*, enumerated as occurring in the Chinese Carboniferous rocks, only one is described by Kayser as distinctly new. The Carboniferous and Devonian brachiopods form the largest group of well-preserved fossils represented, and these include the greatest number of identical species. Plate after plate is made up of forms which are most familiar to every paleontologist of Europe and America. One plate is occupied by figures of *Pentamerus galeatus* Dalm., *Atrypa desquamata* Sowb. (hardly distinct from *A. reticularis*) and *A. aspera* Schloth; the next plate mostly by *Spirifer Verneuili* Murch. (*S. disjunctus* Dav.); one overgrown by *Aulopora tubæformis* Goldf., another carrying *Crania obsoleta*, *Spirorbis omphalodes* and *Cornulites epithonia*, all described by Goldfuss from European specimens. Of the eight Orthides two only are exclusively Chinese. Among the Carboniferous brachiopods nearly all are identical with ours, the first plate which represents them being composed of *Retzia compressa* Meek, *Athyris globularis* Phill., *Spirifer lineatus* and *S. glaber* Martin.

On the next plate are represented *Streptorynchus crenistria*, world-wide in its distribution, our *Meekella striato-costata* Cox and *Terebratula hastata* Sowb. On the next with *Orthis Pecosii* Marcou are several figures of *Syntrielasma hemiplicata* Hall (*Spirifer hemiplicata* Hall, Stansbury's Salt Lake Report), exactly like our peculiar fossil.

An interesting review of the Devonian fauna is given by Kayser

on page 98, from which it appears that of the twenty-eight described species only six are peculiar to China and of these only two, *Nucleospira Tukwenensis* and *Orthis Richthofeni*, are distinctly different from any found elsewhere; the other four Chinese species having representatives in Europe or America, to which they are so closely allied that they may perhaps be only varieties.

On page 199, Kayser begins a still more interesting and suggestive review of the Carboniferous fauna, in which he enumerates 55 species, only ten of which are considered as new. But two new generic names appear in the list, one a coral, *Richthofenia*, first obtained from India, and the other a fish tooth, *Leptodus*, allied to—if indeed it is generically distinct from—*Deltodus cingulatus* N. and W. from Illinois.

Figures of Cambrian trilobites occupy two plates. These are wonderfully like the illustrations of the Primordial fauna given by Angelin from Sweden, Hall from New York and Walcott from Nevada, and evidently represent essentially the same group of organisms.

It has been urged that our representation of the Primordial fauna is too fragmentary to permit generalization in regard to its zoological character and affinities. But we have now gathered from more than twenty localities—some of these antipodal—a group of Primordial fossils which exceed six hundred in number; and of these more than one half are trilobites. The repeated recurrence of this group to the exclusion of other fossils in rocks which hold a fixed relative position in so many and widely separated localities affords demonstrative evidence that they typify the animal life of the age.

The list of genera and species given by Herr Dames has not quite as familiar a look as his plates, for to some well-known forms he has assigned new generic names; for example, to the trilobite best represented in the collections, which has the pygidium armed with strong spines, the penultimate pair being much the longest, he has given the name of *Dorypyge*. Into this genus he brings two trilobites from Utah described by Hall, in King's Fortieth Parallel Report, under the names of *Dikellocephalus quadriceps* and *D.? gothicus*. Similar trilobites, and probably belonging to the same genus, have been found by Walcott in Nevada and by the writer of this notice in Southern Utah at the same geological horizon. *Lyostracus* and *Anamocare* are names which will be less familiar to paleontologists than *Conocephalites* and *Agnostus* with which they are associated in Herr Dames's notes; but they are Angelin's names, given to Primordial trilobites which had before been included in *Crepicephalus* and *Conocephalites*. In *Anomocare* are included such species as *Conocephalites hamulus*, *C. Wisconsensis* and *C. Patersoni* of Hall.

To the fossil plants collected by Baron Richthofen are given 60 of the 269 pages of text and 25 of the 54 plates which compose the volume. As is usual in collections of fossil plants, much of the material representing large organisms of complicated structure

is fragmentary, and to the fragments more significance is sometimes given than is warranted; still a sufficient number of well-marked and characteristic species are represented to make the general conclusions reached by Herr Schenk safe and of great interest. The fossil plants were collected from twelve localities and represent three geological horizons, the Carboniferous, Jurassic and Tertiary.

The Carboniferous species amount to about 40, of which perhaps 30 are sufficiently complete to be satisfactorily identified. Of these, nearly all the most distinctly marked are identical with European Carboniferous species or are very closely allied to them; such as *Calamites gigas*, *Annularia longifolia*, *Neuropteris flexuosa*, *Cyatheites arborescens*, *Sphenophyllum truncatum* (a variety of *S. emarginatum*), *Stigmaria ficoides*, *Cordaites principalis*, etc. Among the new species described is a large *Annularia* with very numerous and contiguous leaves, to which Herr Schenk has given the name of *A. maxima;* but which Zeiller suggests is not distinct from large forms of *A. stellata* Schloth. (= *A. longifolia* Brgt.) (*Annales des Mines*, Oct., 1882). Three species of *Archæopteris* are noticed, for which Geinitz's anticipated name *Palæopteris* is retained. Of these one, "*P. lanceolata* Schenk," figured on page 218, is probably a group of the abnormal folioles which constitute the summit of the pinnæ in some species of *Neuropteris*, as *N. cordata* Brgt. and *N. Rogersorum* Kimball.

The most striking of all the coal plants is a large ovoid leaf like that of a Magnolia, which Schenk has called *Megalopteris*. The name is preoccupied, having been given by Dawson in 1871 to some large Neuropterids with forking pinnæ, and further illustrated by Andrews in the Paleontology of Ohio, Vol. II. The plant is of special interest, and it is to be hoped that Herr Schenk will choose a new name for it, and that its structure and relations will be more fully made out by further collections.

The Mesozoic plants described are from seven localities in China and one in Japan, and they pertain to twenty-nine species, of which nearly all the most distinctly marked have been before obtained from India, the Amoor, Turkestan or Europe. Conspicuous among them are the omnipresent *Asplenium Whitbyense* (*Pecopteris Whitbyensis*), *A. argutulum* and *Podozamites lanceolatus*. They are considered as indicating a Jurassic age for all of the strata which contain them; of this, however, there may be a question, since forms identical with, or allied to, many of those described have been obtained from the Rhætic beds in other parts of the world. If, however, the Rhætic is to be considered as the base of the Jurassic system, and not Triassic—a view which is gaining ground in Europe—there can be no objection made to this conclusion.

The only thing which requires notice among the Jurassic plants is the large frond figured on Plate 51, Fig. 1, regarded by Herr Schenk as *Clathopteris*, but which wants the characteristic rec-

tangular nervation of the genus. The principal nerves are dichotomously forked, and it seems more likely that the fragment represents a new species of *Dictyophyllum*.

A notice of this elegant volume would be incomplete without reference to the exhaustive study by Schwager of the Carboniferous Foraminifera of China and Japan. These form "Fusulina limestones" in the Old World as well as in the New, composing in China, as in Kansas, Utah and California, the greater part of strata many feet in thickness. As in America these singular bodies are sometimes fusiform and sometimes spheroidal. For the former the old name of *Fusulina* is retained, and the spheroidal ones are made to represent a new genus, for which the name *Schwagerina* is chosen by Möller. These are illustrated by four plates and nearly a hundred figures, many of which are highly magnified sections, which will be useful for comparisons with *Nummulites* and with the supposed organic structure of *Eozoön*. J. S. NEWBERRY.

6. *Note on Professor R. D. Irving's paper on the Paramorphic origin of the hornblende of the crystalline rocks of the Northwestern States;* by M. E. WADSWORTH. (Communicated.)—Since Professor Irving's paper in the July number of this Journal appears to imply that no one except Dr. Wichmann has ever studied microscopically the rocks of the Marquette district, and no one except himself advocated the secondary origin of the hornblende in the "greenstones," or the eruptive origin of the diabases, a brief statement of the facts is necessary.

Chas. E. Wright published, in 1873, descriptions of the microscopic characters of a suite of Marquette rocks;* while a more extended study by myself was published in 1880.† In my work the secondary origin of the hornblende in the "greenstones" was distinctly advocated and the rocks classified accordingly.‡ The olivine-bearing character of part of the diabases was then first pointed out, and the supposed serpentine of Presque Isle was first shown to be a peridotite of the lherzolite variety; also the trachytic and rhyolitic nature of the Keweenaw felsites, since advocated by Irving. The eruptive origin of the diabases as held by Messrs. Foster and Whitney was also clearly proved by figures and sections.

Since we agree on these points and since a copy of my published paper was sent him at his special request, it is difficult to understand why Professor Irving should ignore my work, and publish the same views as original with himself.

Cambridge, Mass., July 10, 1883.

7. *Earthquakes of September* 7, 1882, *in Central America.*—A letter from Mr. A. Ernst, of Caracas, in the July number of this Journal calls attention to an error in my last Notice of American Earthquakes (p. 357, May, 1883). He states that this earthquake was *not* felt at Caracas as stated by me. The state-

* Geol. Mich., 1873, ii, 215–231. † Bull. Mus. Comp. Zool., 1880, vii, 28–76.
‡ l. c. pp. 37, 39, 42, 46, 70, etc.

ment was made on the authority of an article in the Atlantic Monthly (March, 1883, p. 397), describing the personal experience of the writer (H. D. Warner), in an earthquake occurring at 2.20 A. M., September 7. The year was not given, but it was a natural inference that it referred to the shock felt in adjacent districts at 3.20 A. M., September 7, 1882. On looking again at the article I see that it *might* refer to some previous year, and I presume that is the fact. Probably Mr. Ernst could tell what year it was.

The correction needed to the notice on page 357 will then be to *erase all reference to Ecuador and Venezuela.*

May I take this opportunity to say also that, as my notes are necessarily based largely upon newspaper notices, which are not always reliable, I shall be glad to receive corrections to my published lists from any persons who may have better information; or, better still, I shall be glad to receive and acknowledge any reliable notes sent me in advance. My permanent address is as below. C. G. ROCKWOOD, Jr., Princeton, N. J., U. S. A.

July 13, 1883.

8. *Brief notices of some recently described minerals.*—EMPHOLITE: Described by Igelström as occurring in radiated and fibrous aggregates of very minute but well-formed crystals. The crystals have been examined by Bertrand and shown to be orthorhombic, and near diaspore in form, having a prismatic angle of 128° to 130°; the planes 110 (*I*), 120 (*i*-2), 130 (*i*-3̆), 010 (*i*-1̆); cleavage, perfect parallel 010. The optic axes lie in the brachydiagonal section, the acute positive bisectrix parallel to the shorter lateral axis, $2V=85°$. Hardness, about 6. Color, white, becoming yellowish on exposure to the air from oxidation of the iron. Infusible, and does not decrepitate, and very slightly, if at all, attacked by acids. Two analyses gave—

	SiO_2	Al_2O_3	MgO, CaO, FeO	H_2O
1.	51·70	31·52	4·60	12·18=100
2.	47·86	33·60	5·70	12·84=100

These results the author corrects for the 16 p. c. gangue intermixed, and obtains

	SiO_2	Al_2O_3	MgO, CaO, FeO	H_2O
1.	52·3	30·5	3·4	13·8
2.	48·8	33·3	3·3	14·6

The mineral is essentially a hydrous silicate of alumina, but owing to the impurity of the material analyzed the exact composition must be regarded as very doubtful. It occurs mixed with cyanite and in cavities in schistose damourite and in quartz; from the Hörrsjöberg, Wermland, Sweden.—*Bull. Soc. Min.*, vi, 40, 1883.

IGELSTRÖMITE, SILFBERGITE and MANGANHEDENBERGITE are manganese minerals, described by Mats Weibull, from Vester-Silfberg, in the Norrbärke parish, Sweden. Igelströmite occurs

in grayish-black crystalline masses of irregular texture, sometimes almost homogeneous, and again mixed with a carbonate of lime and manganese, and magnetite. Cleavage in two directions, making an angle of 131° with each other, and a third, indistinct, at right angles. Specific gravity, 4·17. Luster, vitreous to greasy. Translucent with yellowish color. An analysis gave—

SiO_2	FeO	MnO	Mgo	$CaCO_3$
29·94	46·88	18·83	3·01	1·14=99·80

This corresponds to the chrysolite group, the mineral being intermediate between fayalite (Fe_2SiO_4) and knebelite $FeMnSiO_4$, it having the ratio of Fe : Mn=2 : 1. The same mineral has been previously described by Igelström under the name of knebelite, of which it is properly a variety. (The name igelströmite has been used by Heddle; see 3d Append. Syst. Min., p. 99.)

Silfbergite is a honey-yellow mineral resembling actinolite. It occurs in bladed crystals and crystalline aggregates showing two prismatic cleavages like hornblende, also in nearly compact masses. Luster, vitreous. Hardness, 5·5, and specific gravity 3·446. Translucent, slightly dichroic. An analysis gave—

	SiO_2	FeO	MnO	MgO	CaO	ign
(3)	48·83	30·49	8·34	8·39	1·74	0·44=98·23

This brings it near anthophyllite, from which it seems to differ only in its percentage of manganese. Silfbergite occurs with the above described igelströmite.

Manganhedenbergite is a grayish-green pyroxenic mineral, very near hedenbergite, but characterized by its containing a few per cent of manganese. Hardness 5, and specific gravity 3·55. An analysis gave—

SiO_2	FeO	MnO	CaO	MgO	Na_2O, K_2O
48·29	24·01	6·47	17·69	2·83	0·22=99·51

Together with the above minerals three other manganesian species are described, a manganese garnet, a carbonate of lime and manganese, and wad.—*Geol. För. Förh.*, vi, 499, 1883.

III. Miscellaneous Scientific Intelligence.

1. *A visit to Ceylon*, by Ernst Hæckel. Translated by Clara Bell. 338 pp. 8vo. Boston, 1883. (S. E. Cassino & Co.) —Hæckel, in this book of travels, is not the investigating and theorizing biologist which his name brings before the mind, but the man of general culture—science not excluded—finding delight in nature and in the people about him, as new scenes opened to view in his journeyings. His descriptions—which embrace all kinds of subjects that suggest themselves to an observing traveler, and much that only one with an eye quickened by science would see—are always excellent and often amusing. The tropi-

cal forests and landscapes, and the coral and other scenery of the waters, gave him great pleasure, and the reader derives much of that pleasure from the perusal of his glowing but never overdrawn sentences. The volume, in its English dress, is an attractive one in all respects, and not the least so for the portraiture it gives through its pages of the eminent biologist.

2. *Handbuch der Klimatologie;* von Dr. JULIUS HANN, Direktor der met. Zentralanstalt, und Professor an der Univ. in Wien, etc. 764 pp. 8vo. Stuttgart, 1883: (J. Engelhorn).—This volume is one of the series constituting the Bibliothek Geographischer Handbücher, edited by Professor Dr. F. Ratzel, and several eminent collaborators. It is a valuable work, giving in compact form a large amount of information in regard to the climates of the different parts of the earth. It opens with an introduction devoted to a statement of the various climatic elements; that is, the temperature and its changes, the humidity of the atmosphere, and the precipitation of rain, snow or dew, the clouds and fog, winds, atmospheric pressure, evaporation, and composition of the air. The work proper is divided into two parts, the first containing a discussion of general climatology considered under the two heads of *the solar climate*, or the climate of the earth as determined simply by the amount of solar radiation and the character of the earth's atmosphere. The second head is the *physical climate*, or the actual terrestrial climate as influenced by the modifying causes of the varied character of the earth's surface, of land and water with the accompanying currents of air and sea, and the unequal altitudes of different parts of the earth above the sea surface. Under the second head the characteristics of land and sea climates on the one hand and of the mountain climate on the other, are discussed at length. This portion of the work, comprising about one-sixth of the whole, contains much useful matter; the causes of the varying temperature over land and sea, the influence of the continents on the moisture of the atmosphere and the amount of precipitation, the influence of the land on the wind in the different seasons, the influence of ocean currents on the climate, of mountain chains upon the precipitation of rain or snow are some of the topics which are discussed. The second part of the volume is devoted to special climatology or the description of the climate of the different parts of the earth, first for tropical regions, then for the temperate zones, and finally for polar regions. The considerable space, upwards of 500 pages, given to this portion of the subject allows the author to present the climatic peculiarities of the various regions in considerable detail, and whether the reader is interested in the gold coast of Africa, in Alaska, or the Sandwich Islands, he will find here a good summary of the results of past observations.

3. *Manual of Taxidermy: a complete guide in collecting and preserving Birds and Mammals;* by J. C. MAYNARD. 112 pp. 12mo, with illustrations. Boston, 1883. (S. E. Cassino & Co.)—

The author gives his facts and advice briefly as well as clearly, and hence brings the whole within a comparatively few pages. The work takes up, in order, the subjects of collecting by trapping, shooting and in other ways; skinning and preserving; making skins—under which head varying directions are given according to the tribe; labelling, mounting, etc. Birds are first considered, and after these, Mammals; and much more briefly, Reptiles, Batrachians and Fishes.

4. *Dredging in* 1880 *by the U. S. Coast Survey Steamer Blake: Report on the Ophiuroidea;* by THEODORE LYMAN. Vol. x, No. 6, of the Bulletin of the Museum of Comparative Zoology, pp. 227–286, with 8 plates. Cambridge, May, 1883.

5. *Royal Society.*—Professor Huxley has been elected President of the Royal Society, in place of Mr. Spottiswoode, deceased.

6. *Professorship of Chemistry in the University of Virginia.*—A circular letter from Dr. James F. Harrison, Chairman of the Faculty of the University, states that an election to fill the Chair of Chemistry, including General and Industrial Chemistry, made vacant by the resignation of the present incumbent, will be held by the Board of Visitors of the University of Virginia, on the 11th of September, 1883. The salary of the Professor will be $3,000, payable in three equal parts, on the 15th of October, January and April, respectively, with a commodious house, recently built and provided with modern conveniences, rent free. Applications, with testimonials, must be addressed to "The Rector and Visitors," P. O. University of Virginia, Albemarle County, Virginia.

7. *American Association.*—The thirty-second meeting of the American Association, to be held at Minneapolis, will open on the 15th of August, under the presidency of Professor C. A. Young, of Princeton. A list of the officers of the meeting is given in volume xxiv of this Journal on page 304. The rooms of the Association will be at the State University. Professor N. H. Winchell is the Secretary of the Local Committee, and from him the circular of this committee, giving hotel charges, a list of the railroads (twenty or more in number) which will give reduced rates, and other information of value to the members, can be obtained. An illustrated guide to the city of Minneapolis will be distributed to the members.

Express packages containing apparatus, specimens, maps, books and other articles designed for use in the meetings of the Association, will be forwarded by the American Express Company and delivered free of charge at the State University. They should be addressed to the *care of Professor J. A. Dodge,* to whom all correspondence relating to the same should be sent.

All matters relating to membership, the presentation of papers, payment of assessments, etc., and business to come before the Association will be attended to by the *Permanent Secretary* at *Salem, Mass., up to August 8th;* and after this date, until the adjournment, at the Nicollet House, Minneapolis. Professor Put-

nam reports that by the generosity of General William Lilly, the Nashville volume of the Proceedings has been reprinted, and by the liberality of another member the St. Louis volume is now being reprinted, while the several smaller gifts of a number of members will be held until a sufficient sum is subscribed to reprint volume 2. The copies of volume 24 are now nearly exhausted and further subscriptions to the reprinting fund are greatly desired. Copies of 2, 24 and 27 will be received in exchange for other volumes, or for assessments at the rate of a dollar a volume. Members can complete their sets of Proceedings at the rate of one dollar a volume, if ten or more volumes are ordered at one time; under ten volumes the price is $1.50 a copy. The quarto memoir on Fossil Butterflies by S. H. Scudder, printed by the donation of Mrs. Elizabeth Thompson, is furnished by the Association at $2.00 a copy; the Transactions of American Geologists and Naturalists, 8vo, 1843, at $3.00 a copy.

Liberal arrangements are made for the Association at Minneapolis, and it is confidently expected that the meeting will be a large one.

Obituary.

Sir Edward Sabine, K.C.B., died on the 25th of June, in his 95th year. General Sabine's name is known in science most prominently through his two long series of observations and discussions connected with terrestrial magnetism and the pendulum. Both series were begun over sixty years since, after voyages to the Arctic with Sir John Ross and Sir Edward Parry, and the former was continued until 1872, in which year appeared a paper giving a magnetical survey of the North Polar regions, to serve as a companion to the survey of the South Polar region which had already appeared. General Sabine discovered, simultaneously with Wolf and Gautier, the connection between unusual magnetic disturbances and large declination, and the year or time of maximum sun spots. His election to the Royal Society took place in 1818, when in his 30th year. In 1821, he received from it the Copley medal; in 1849, the Royal medal. In 1861 he was elected President of the Royal Society, which position he held for ten years. In 1869 he was made K.C.B. The Colonial Observatories owed their existence to the efforts of General Sabine, and were for many years under his control.—*From a biographical notice in Nature of July 5th.*

William Spottiswoode, President of the Royal Society, eminent as a mathematician and physicist, and a scholar also in oriental and other languages, died on the 27th of June. He was buried in Westminster Abbey. Mr. Spottiswoode was born in London, Jan. 11, 1825, but was of Scottish family. His earliest mathematical work, "Meditationes Analyticæ," appeared when he was twenty-two years old. After 1870 his attention was divided between physics and mathematics. A complete list of his papers is contained in Nature of April 26.